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WORD

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A STATISTICAL APPROACH TO QUANTITATIVE LINGUISTIC ANALYSIS

DAVID W. REED

1. *Introduction.* In the scientific method generally it is customary to recognize two well-defined types of procedure—qualitative analysis and quantitative analysis. Qualitative analysis is a body of methods by means of which the nature and variety of given phenomena are observed and classified. Quantitative analysis is a body of methods by means of which the magnitude and frequency of such phenomena are observed and classified. Successful quantitative analysis presupposes a certain minimum of qualitative study which sets up a scheme of classification to determine the basic structural units to be examined for magnitude and frequency. For example, it would be impossible to determine the atomic weight of hydrogen—a problem of magnitude—without first having inferred the existence of units called atoms and of certain elements of matter, one of which is termed hydrogen.

In the scientific method of linguistics it is possible to recognize the same two types of analysis—qualitative and quantitative—which may be classed as subtypes of the general scientific methods described above, since the phenomena analyzed are specifically linguistic but the methods of analysis are essentially like those in the other sciences. Quantitative analysis in linguistics shows the same dependence upon previous qualitative study as it does in other fields of science. For example, it would be impossible to determine the relative frequency of the phoneme *p* in a given dialect of American English without first having inferred the existence of units called phonemes, one variety of which is termed *p* in American English.

Linguistic analysis, as ordinarily performed at present, tends to be largely qualitative: the descriptive techniques of phonetics, phonemics, morphology, and syntax aim generally to analyze the nature and variety of linguistic phenomena rather than the magnitude and frequency of such phenomena. As was pointed out, however, qualitative study of this sort is a prerequisite to successful quantitative analysis, so that the present results of qualitative linguistic analysis furnish the best basis upon which quantitative study may rest. It would appear, moreover, that qualitative analysis has been pursued for so long and with such thoroughness that it will now be fruitful for linguists to turn some of their attention to quantitative analysis.

Certain linguistic studies of a primarily quantitative nature have of course been made in the past and are being made at present. Any linguistic text, whether in the ordinary orthography of previous historical periods of a given language

or in a phonemic transcription of actual speech in a contemporary dialect, contains much quantitative as well as qualitative information. A glossary which is compiled from such a text and which lists, by line reference or some similar device, each occurrence of the various linguistic forms in the text makes a large part of the quantitative data more readily available. A further step toward quantitative analysis is taken when the occurrences of linguistic forms in such a comprehensive glossary or in the text itself are counted and tabulated in terms of raw totals. A still closer approach to quantitative analysis is made if the raw totals resulting from counting the frequency of linguistic forms are reduced to expression in terms of percentages of some larger unit.

Quantitative statements of the types mentioned, however, should properly be termed quantitative pre-analyses,[1] since they are incapable of providing an accurate indication of whether a given linguistic form is more frequent than another in a language or dialect as a whole, or whether the form merely happens to be more frequent than others in the text under consideration. The goal of quantitative linguistic analysis is, clearly, to obtain an indication of the relative frequency or magnitude of a linguistic form in a whole dialect at a given time; therefore it is necessary to discover and apply further techniques of study. Statistical techniques, particularly those pertaining to the theory of sampling, have been worked out to provide accurate indications on just such matters. If we are to arrive at reliable measures of relative frequency or magnitude, these techniques should be applied to the raw quantitative data of language.

Before consideration can be given to the nature and application of such techniques, however, it will be necessary to clear up one other matter. It will have been noted that quantitative linguistic analysis has been defined as a body of methods by means of which the *magnitude* and *frequency* of linguistic phenomena are observed and classified—yet all of the linguistic examples given above have been concerned with frequency rather than with magnitude. The data of magnitude in linguistics happen to be almost exclusively the province of instrumental phonetics (including such matters as the intensity of stress, the duration of speech sounds, and so forth). The data of frequency in linguistics, on the other hand, are to be drawn from almost all divisions of the field. Because the data of

[1] For examples of the various types of quantitative pre-analysis, reference may be made to editions of early texts, such as those of the EETS or the SATF (which often contain comprehensive glossaries and occasionally give raw totals for the frequency of linguistic forms) to any of a number of German dissertations and studies on the language or some specific phase of the language of an early text (which often give raw totals for the frequency of linguistic forms) and to such sources as Samuel Moore, Loss of Final *n* in Inflectional Syllables of Middle English, *Language* 3.232–59, and Charles C. Fries, *American English Grammar*, New York 1940 (both of which give data of linguistic frequency in terms of percentages, the former without, the latter with an indication of the size of the sample). Perhaps the most determined effort to study the quantitative data of language is to be found in the works of George K. Zipf, *Relative Frequency as a Determinant of Phonetic Change*, Harvard Studies in Classical Philology 40.1.95 (1929), *Selected Studies in the Principle of Relative Frequency in Language*, Cambridge, Mass., 1932, and *The Psycho-Biology of Language; An Introduction to Dynamic Philology*, Boston 1935. These studies however do not apply statistics to the problem of sampling.

linguistic frequency appear not to have been analyzed so thoroughly as those of linguistic magnitude, and because their adequate analysis should be a matter of interest to all linguists, the statistical portions of the present paper will be devoted to an exposition of certain devices which are of aid in analyzing the data of frequency.[2]

The two elementary statistical devices which will be presented are those which may aid in answering the following questions in quantitative linguistic analysis: (1) How much evidence should be collected in order to make a valid analysis of the frequency of linguistic forms? (2) When may quantitative differences in linguistic material be considered significant? The specific mathematical formulas to be employed will be developed inductively in connection with simple, hypothetical examples of quantitative linguistic data. Consideration will then be given to a few specific applications of each statistical technique to further linguistic data. Finally the range of application of such devices to linguistic data and the sort of contributions that such devices can make to linguistic knowledge will be considered.

2. *Two Important Statistical Devices.* Up to this point, sampling has been mentioned rather prominently, and it is a fact that the practical linguist seldom if ever works with evidence which is complete in the quantitative sense. His texts are often qualitatively complete in that they provide examples of all the structural units of the language being studied, but in order to be quantitatively complete the texts would need to reproduce every linguistic form uttered or written at a given period in the language within a particular speech community or area. Such quantitatively complete evidence is impossible to collect, and even if it were collected, would be practically impossible to analyze. The linguist, therefore, studies samples of evidence which he hopes to be more or less reliable quantitatively. But if the data from such samples are assumed to be capable of yielding quantitatively significant results, the sampling procedure must be safeguarded by analyzing the data in the light of statistical theory.

The theory of sampling is, briefly, that a sample of forms from a larger body of evidence tends to show almost all of the same features in approximately the same proportions as does the larger body of evidence. Thus, if a linguist is working with a short phonemic transcription of contemporary American English speech, it is more than likely that he will find the transcription to contain examples of the phonemes *p* and *b*, which of course occur in the language as a whole. Moreover, if the investigator counts the occurrences of the two phonemes in his text and arrives at a total of 100, of which fifty are *p* and fifty are *b*, he has an indication that these phonemes are of approximately equal frequency in the stream of speech of the dialect as a whole.

It should be noted of course that the proportions of features in any given sample are only *approximately* the same as the proportions of these features in the larger body of evidence from which the sample is drawn. To continue the previous example, if the investigator collects more material and counts the next

[2] Termed 'statistics of attributes' by G. U. Yule in *An Introduction to the Theory of Statistics*, Sixth Ed., London 1922.

100 occurrences of *p* and *b* in his new texts, it is quite possible that he will find more than fifty of one or the other, although it is also possible that he will again find fifty and fifty. If four or five such samples are counted, it is possible even that each sample will have a different proportion of *p* and *b*. In such circumstances it becomes desirable to know how closely the proportions in a given sample may be expected to approximate the proportions in the larger body of evidence from which the sample was drawn. One statistical device for calculating this degree of approximation is the standard error of a proportion.

2.1 *The Standard Error of a Proportion.*

2.11 *Calculation of the Standard Error of a Proportion.* Statisticians have exhaustively studied the reliability of various types of samples, by investigating the empirical evidence of coin-tossing and dice-throwing, in which the proportions of heads and tails which will appear when the coin is tossed or of the various numbers which will appear when the dice are thrown may be assumed a priori. It has been clearly established that the reliability of a sample is dependent on two factors: first, the number of items counted in the sample, and, second, the size of the proportions of attributes counted in the sample.

It seems entirely consistent with common-sense reasoning that the larger a sample is, the more likely it is to approximate the conditions of the still larger body of evidence from which it is drawn. Thus, the investigator who wished to determine the proportion of *p* to *b* in a given dialect of American English could make a more accurate estimate after counting one thousand occurrences of bilabial stops in a running text than he could after counting only one hundred; and he could make an even more accurate estimate after counting the data of 10,000 such occurrences. This much is self-evident. But the evidence in samples of coin-tossing and dice-throwing indicates that the margin of error does not decrease in direct proportion to the increase in the size of the sample. In other words the error in a sample of 10,000 items is not one-tenth of the error in a sample of 1,000 items, nor one-hundredth of the error in a sample of 100 items. Rather, the margin of error decreases in direct proportion to the *square root* of the increase in the size of the sample. Thus, if everything else is equal, a sample of 10,000 items will have an error equal to $\frac{1}{\sqrt{10}}$ times the error in a sample of 1,000 items or equal to one-tenth $\left(i.e., \frac{1}{\sqrt{100}}\right)$ the error in a sample of 100 items.

As has already been pointed out, the reliability of a sample is not wholly dependent on the number of items in the sample, but depends also upon the size of the proportions of attributes in the sample. Empirical evidence indicates that the reliability of a sample becomes greater as the proportion of either of the attributes being counted approaches 100 percent or zero percent, and that it becomes less as the proportion of either attribute approaches fifty percent. Thus a sample of 100 consisting of 90 *p* and 10 *b* in Language A is more reliable than a sample of 100 showing 50 *p* and 50 *b* in Language B. Stated differently, the proportions in the first sample probably represent a closer approximation to the

actual situation in Language A as a whole than do the proportions in the second sample to the actual situation in Language B as a whole.

The reasons for this fluctuation in reliability in terms of the nature of the proportions are perhaps not immediately apparent to the faculty of common sense, but the empirical evidence of coin-tossing and dice-throwing establishes the fact of fluctuation beyond question: The error varies directly as the square root of the product of the two proportions. In the above example, in Language A, where the proportion of *p* to *b* was 90 percent to 10 percent, the product of the proportions is 900, and the square root of the product is 30. In Language B, where the proportion was 50 percent to 50 percent, the product of the proportions is 2,500, and the square root of the product is 50. If everything else is equal, the error of the sample of forms from Language A is three-fifths (*i.e.*, thirty-fiftieths) of the error of the sample of forms from Language B.

The standard error formula $SE = \sqrt{\frac{pq}{N}}$ takes into account these two factors which bear upon the reliability of a sample. In the formula SE represents standard error; p represents the proportion (percentage of frequency) of one of the items being counted, such as *p*-phonemes in the example above; q represents the proportion of the other item being counted, such as *b*-phonemes in the example above; and N represents the total number of items counted in the sample. The elements to the right of the equal sign are those derived from the empirical evidence considered above. The $\sqrt{pq}$ is in the numerator because it varies directly with the standard error; the $\sqrt{N}$ is in the denominator because it varies inversely with the standard error.

If the standard error formula is applied to the proportional data of a given sample, the resulting standard error will be a percentage figure ranging somewhere between zero and 35.4 percent. A standard error of zero, indicating the most reliable sample possible, results when either of the proportions of items being counted is zero. Thus, if a sample of one thousand bilabial stops from a given language shows one thousand instances of *p* and zero instances of *b*, common sense tells us that we are not likely to find any instances of *b* in the next thousand. Worked out in terms of our formula, there is a standard error of zero. $SE = \sqrt{\frac{1.00 \times 0}{1000}}$, or SE = 0 percent. On the other hand, common sense also tells us that the situation offering the greatest possibilities for further deviation would be one in which our sample consisted of only two forms, one of which was *p* and the other *b*.[3] Under these circumstances we would arrive at a standard error of 35.4 percent. $SE = \sqrt{\frac{.5 \times .5}{2}}$, or $SE = \sqrt{.125}$, or finally SE = 35.4 percent.

[3] A sample consisting of a single form would have a standard error of zero, since $SE = \sqrt{\frac{1.00 \times 0}{1}}$, or SE = 0 percent, yet it is not a reliable sample and would certainly not be used unless very special circumstances obtained.

Before the applications of the standard error formula to quantitative linguistic data are considered fully, it might be well to calculate the standard error of several hypothetical samples, in order to get some idea of the values that it may assume for different sorts of samples. Let us consider again the first example that was presented above—that of a sample of one hundred bilabial stops, of which fifty are *p* and fifty are *b*. Substituting these data into the standard error formula, we get the following result: $SE = \sqrt{\frac{.5 \times .5}{100}}$, or $SE = \sqrt{.0025}$, or finally SE = 5 percent.

It was stated earlier that a sample of one hundred bilabial stops of which 90 are *p* and ten are *b* would prove more reliable than one of which 50 are *p* and 50 are *b*. If we calculate the standard error of the proportion 90 to 10 in such a sample, we get the following result: $SE = \sqrt{\frac{.9 \times .1}{100}}$, or $SE = \sqrt{.0009}$, or finally SE = 3 percent. Thus the smaller standard error reflects the fact that a sample becomes more reliable as either proportion of the items being counted approaches zero.

It was also stated earlier that a sample of 10,000 items is far more reliable than a sample of 100 items if the proportions in the samples are the same. If we calculate the standard error of a sample of 10,000 bilabial stops of which exactly 5,000 are *p* and 5,000 are *b*, we get the following result: $SE = \sqrt{\frac{.5 \times .5}{10{,}000}}$, or $SE = \sqrt{.000025}$, or finally SE = 0.5 percent. Thus the smaller standard error reflects the fact that a sample becomes more reliable as the total number of items counted becomes greater.

2.12 *Specific Uses of the Standard Error of a Proportion.* The nature of the standard error of a proportion having been explained and illustrated, it should be apparent that the device indicates accurately the degree of reliability of a given proportion which occurs in a particular sample. It seems reasonable that, whenever a percentage figure is used to express the frequency of a given linguistic form, there ought also to be some indication of the reliability of that percentage figure. Standard error provides a simple, concise method of indicating this reliability and could profitably be employed in this connection. For example, when the investigator completes his count of 100 bilabial consonants from a phonemic transcription of American English speech, he need report only 'Proportion of bilabial stops which are *p*—50 percent; SE—5 percent.' Thus, although the theory underlying the standard error is relatively complex, the practical use of the device to indicate the degree of reliability of a proportion is quite simple.

It may be objected that, rather than employing an unfamiliar device such as standard error, the investigator of quantitative linguistic data should merely state the number of items in his sample and the proportions of the attributes counted, allowing others to conclude what the reliability of the data may be. We have seen, however, that the reliability of a proportion depends on several complex relationships between the number of items in the sample and the nature of the proportions themseves. The very complexity of the relationships renders it unlikely that a casual observation of the raw data will give any very clear idea

of the reliability of the proportions involved. The standard error, however, indicates that reliability concisely and accurately. It would seem, therefore, that the use of standard error is definitely preferable to a mere indication of the size of the sample upon which a proportion is based.

A further use of the standard error device may be considered at this point. So far, examples have been limited to the very simple case of counting the proportion of bilabial stops that are *p* and *b*. It is, of course, rare that a problem involving the frequency of only two phonemes would be investigated; the hypothetical example was selected merely to afford a simple illustration of the technique of calculating the standard error. Suppose that the purpose of the investigation is to determine relative frequency of *all* the phonemes in a given dialect. The results of such a study are frequently presented by stating that a running text containing, say, 1,000 phonemes was investigated and that it was found that 50 phonemes or 5 percent of the total sample were *p*, that 45 phonemes or 4.5 percent of the total sample were *b*, that 5 phonemes or 0.5 percent of the total sample were *ŋ*, and so forth. From what has already been noted about the reliability of proportions, however, it will be observed that in this example, since the size of the sample is constant for all the phonemes counted, the percentage of 0.5 for *ŋ*, because it is closer to zero, will be considerably more reliable than the percentage of 5 for *p* (see above under 2.11). If any comparison is to be made between the proportions of the various phonemes as indicated in these data, it must be recognized that some of the proportions are more reliable than others. This may be accomplished by listing the standard error of each proportion, thus: 'frequency of *p*—5 percent, SE—0.69 percent; frequency of *b*—4.5 percent, SE—0.65 percent; frequency of *ŋ*—0.5 percent, SE—0.22 percent.'

A more economical procedure, however, would be to decide in advance the degree of reliability to be achieved in each proportion. A given standard error, such as 1.0 percent or 0.5 percent, to which each proportion will be made to conform, may be chosen arbitrarily. The size of the sample will then vary considerably for each different phoneme counted,[4] but the reliability will remain constant. In presenting the results of a study carried out along these lines, it will be possible to indicate in the introduction that the standard error of each proportion is 1.0 percent, or 0.5 percent, as the case may be. The proportions themselves may be presented without any necessary reference to further factors such as the varying size of the sample in the case of each proportion, or to a varying degree of reliability of each proportion as in the previous type of analysis. Moreover, such proportions with a constant standard error may be compared with one another at a glance much more satisfactorily than may proportions with varying standard errors.

2.2 *The Standard Error of the Difference Between Two Proportions.*

2.21 *Calculation of the Standard Error of the Difference.* Up to this point the standard error of a single proportion has been presented as a device for deter-

[4] In the example above it would be necessary to count a sample of 1901 phonemes, 5 percent, or 95, of which were *p*, but a sample of only 199 phonemes, 0.5 percent, or one, of which were *ŋ*, to obtain figures with a standard error of 0.5 percent in both cases.

mining how large a sample of evidence ought to be collected for analysis and as a device for describing the reliability of a proportion from a given sample of evidence after it has been collected and analyzed. A further and perhaps even more important use of standard error may be found in its applications to the comparison of two or more proportions.

It is usually desirable, after collecting quantitative data and calculating the proportions of various attributes which are present in the sample, to compare one proportion with another in order to learn whether any significant differences exist between the proportions. The differences between sampling proportions are significant if they may be taken to reflect a real difference of proportions in the larger body of evidence from which the sample is taken. Thus in the hypothetical example of counting the frequency of phonemes in a given dialect of American English, after discovering that 5 percent of a running text of 1,000 phonemes were *p* while only 0.5 percent of the same phonemes were *ŋ*, we should want to know whether the difference between the two proportions is indicative of a real difference between the frequencies of the two phonemes in the language as a whole or whether it is due merely to a fluctuation in the samples.

It was calculated that the standard error of the 5 percent frequency of *p* was 0.69 percent while that of the 0.5 percent frequency of *ŋ* was 0.22 percent. It will also be remembered that these standard errors are a measure of the deviation which may be expected to exist between the proportions in the sample and the actual proportions in the language as a whole. In order to compare the two proportions with one another, however, it will be necessary to calculate the standard error of the *difference* between the two proportions. The standard error of the difference is based upon the two standard errors of the individual proportions and, as might be expected, is usually larger than the standard error of either individual proportion. Statisticians, again working from the empirical evidence of coin-tossing and dice-throwing, have discovered that the standard error of the difference between two proportions is equal to the square root of the sum of the squares of the standard errors of the two proportions. That is, expressed as a formula, $SE_{dif} = \sqrt{SE_1^2 + SE_2^2}$. Applying this formula to the data from the hypothetical case above, we obtain $SE_{dif} = \sqrt{(.0069)^2 + (.0022)^2}$ or $SE_{dif} = \sqrt{.00004761 + .00000484}$, or $SE_{dif} = \sqrt{.00005245}$, or, finally, $SE_{dif} =$ 0.72 percent.

This calculated standard error of the difference between the two proportions is a measure of the deviation which might be expected to occur between the two proportions as a mere fluctuation in the samples (*i.e.*, it is a measure of the purely chance deviation which may be expected to occur). If we compare this measure of chance deviation with the actual deviation of 4.5 percent between the two proportions (5 percent minus 0.5 percent) we see that the actual deviation amounts to 6.25 standard errors $\left(\frac{4.5}{0.72}\right)$. Expressed in another way, the actual difference in this case is six and one-quarter times greater than the standard error of the difference.

In order to interpret this figure of 6.25, statisticians make use of further empirical evidence. They have discovered that an actual deviation equal to or

greater than 1.00 standard error of the difference will occur in as many as 31.74 percent of proportions in a normal distribution. An actual deviation equal to or greater than 2.00 standard errors will occur in only 4.56 percent of proportions in a normal distribution; a deviation equal to or greater than 3.00 standard errors will occur in only 0.27 percent of proportions in a normal distribution; and so forth. An actual deviation as great as 6.25 standard errors is so rare that it is not listed in most charts, but it may be noted that a deviation as great even as 5.0 standard errors will occur in only 0.00005734 percent of proportions in a normal distribution! In other words, a deviation as great as 5.0 standard errors would occur on a basis of pure chance only once in about two million samples, and a deviation of 6.25 standard errors would occur considerably less frequently. Such a great deviation can scarcely be due to chance.

The percentage of probability of occurrence of a given deviation expressed in terms of standard error units cannot be conveniently calculated by formula. For this reason statisticians prepare tables of correspondences based upon the empirical evidence of normal distributions of samples. Such information on probability is provided in Table One[5] of the present study. The table is to be used as follows: After one has determined the actual deviation in terms of standard error units, this figure is located by cross reference between the column at the left of the table and the row at the top of the table. The percentage figure thus found in the body of the table at the intersection of the appropriate column and row indicates the probability, on a basis of pure chance, of a deviation between two proportions (in terms of standard error units) as large as or larger than the deviation actually found. A deviation as large as 6.25 standard error units, calculated for the two hypothetical proportions in the above example, is not listed in the table. For the purpose, however, of checking the technique of employing the chart, one may look up the probability of occurrence of a deviation as large as or larger than 1.37 standard errors, which will be found to be 17.06 percent.

It will be noted in the chart that when there is no deviation between two proportions (*i.e.*, 0.00 standard error units deviation) the probability is 100.0 percent. This listing merely indicates the obvious fact that all pairs of proportions will show either zero deviation or a deviation larger than zero. At the other end of the scale the probability approaches zero for the largest deviations, but it never quite reaches zero, no matter how large the deviation may be. These listings in the table reflect the fact that any deviation, however large, is theoretically possible on a basis of pure chance, although the probability of very large deviations is indeed slight.

The important practical point to note in the detailed data of Table One is that a percentage of probability above 5.00 is usually taken to indicate a mere chance deviation; a percentage of probability below 5.00 is usually taken to indicate a strong likelihood that some factor other than chance is responsible for the deviation; and a percentage of probability below 1.00 is usually taken to indicate almost the certainty that some factor other than chance is responsible

[5] Adapted from Henry E. Garrett, *Statistics in Psychology and Education* 467 (Second Ed., New York 1939), table 15, with permission of Longmans, Green, and Co.

TABLE ONE

The Probability of Occurrence of Standard-Error-Unit Deviations

AD/SE	.00	.01	.02	.03	.04	.05	.06	.07	.08	.09
0.0	100.0	99.20	98.40	97.60	96.80	96.02	95.22	94.42	93.62	92.82
0.1	92.04	91.24	90.44	89.66	88.86	88.08	87.28	86.50	85.72	84.94
0.2	84.14	83.36	82.58	81.80	81.04	80.36	79.48	78.72	77.94	77.18
0.3	76.42	75.66	74.90	74.14	73.38	72.64	71.88	71.14	70.40	69.66
0.4	68.92	68.18	67.44	66.72	66.00	65.28	64.56	63.84	63.12	62.42
0.5	61.70	61.00	60.30	59.62	58.92	58.24	57.54	56.86	56.20	55.52
0.6	54.86	54.18	53.52	52.86	52.22	51.56	50.92	50.28	49.66	49.02
0.7	48.40	47.78	47.16	46.54	45.92	45.32	44.72	44.12	43.54	42.96
0.8	42.38	41.80	41.22	40.66	40.10	39.54	38.98	38.44	37.88	37.34
0.9	36.82	36.28	35.76	35.24	34.72	34.20	33.70	33.20	32.70	32.22
1.0	31.74	31.24	30.78	30.30	29.84	29.38	28.92	28.46	28.02	27.58
1.1	27.14	26.70	26.28	25.86	25.42	25.02	24.60	24.20	23.80	23.40
1.2	23.02	22.62	22.24	21.86	21.50	21.12	20.76	20.40	20.06	19.70
1.3	19.36	19.02	18.68	18.36	18.02	17.70	17.38	17.06	16.76	16.46
1.4	16.16	15.86	15.56	15.28	14.98	14.70	14.42	14.16	13.88	13.62
1.5	13.36	13.10	12.86	12.60	12.34	12.12	11.88	11.64	11.42	11.18
1.6	10.96	10.74	10.52	10.32	10.10	9.90	9.70	9.50	9.30	9.10
1.7	8.92	8.72	8.55	8.36	8.18	8.02	7.84	7.68	7.50	7.34
1.8	7.18	7.02	6.88	6.72	6.58	6.44	6.28	6.14	6.02	5.88
1.9	5.74	5.62	5.48	5.36	5.24	5.12	5.00	4.88	4.78	4.66
2.0	4.56	4.44	4.34	4.24	4.14	4.04	3.94	3.84	3.76	3.66
2.1	3.58	3.48	3.40	3.32	3.24	3.16	3.08	3.00	2.92	2.86
2.2	2.78	2.72	2.64	2.58	2.50	2.44	2.38	2.32	2.26	2.20
2.3	2.14	2.08	2.04	1.98	1.92	1.88	1.82	1.78	1.74	1.68
2.4	1.64	1.60	1.56	1.50	1.46	1.42	1.38	1.36	1.32	1.28
2.5	1.24	1.20	1.18	1.14	1.10	1.08	1.04	1.02	0.98	0.96
2.6	0.94	0.90	0.88	0.86	0.82	0.80	0.78	0.76	0.74	0.72
2.7	0.70	0.68	0.66	0.64	0.62	0.60	0.58	0.56	0.54	0.52
2.8	0.52	0.50	0.48	0.46	0.46	0.44	0.42	0.42	0.40	0.38
2.9	0.38	0.36	0.36	0.34	0.32	0.32	0.30	0.30	0.28	0.28
3.0	0.27	0.26	0.26	0.24	0.24	0.22	0.22	0.22	0.20	0.20
3.1	0.20	0.18	0.18	0.18	0.16	0.16	0.16	0.16	0.14	0.14
3.2	0.14									
3.3	0.10	3.5	0.04	4.0	0.0063					
3.4	0.06	3.6	0.02							
		3.7	0.02	4.5	0.00068					
		3.8	0.01							
		3.9	0.01	5.0	0.00005734					

for the deviation.[6] To return to the original problem used to illustrate the application of this statistical device, the deviation in frequency of 6.25 standard errors between 50 occurrences of p and 5 occurrences of η in a sample of 1000

[6] R. A. Fisher, *Statistical Methods for Research Workers* 128–33, 230–8 (Sixth Ed., Edinburgh 1936).

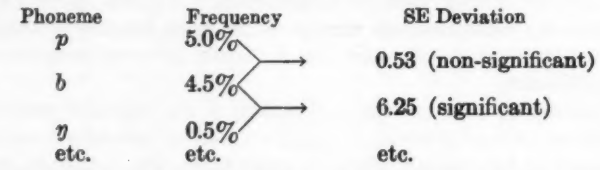
phonemes must almost certainly be due to the operation of some factor other than chance, since such a chance deviation would occur considerably less often than once in two million pairs of proportions. Therefore, given these hypothetical data, one must conclude that p occurs more frequently than in y the whole dialect in question. In short the deviation between proportions in the sample is significant.

Before considering some specific applications of the standard error of the difference between two proportions, it might be well to calculate it consecutively for another set of hypothetical data, in order to clarify further its nature and method of calculation. We shall return to a pair of proportions mentioned previously: In a sample of 1000 phonemes from a given dialect of American English, the investigator notes 50 occurrences of p and 45 occurrences of b. He wishes to know whether the actual deviation of 0.5 percent (5.0 percent minus 4.5 percent) in these proportions is significant. The first step in seeking a solution to the problem is to calculate separately the standard error of each proportion. This calculation was made above, and it was found that the standard error of the 5.0 percent frequency of p is 0.69 percent and that of the 4.5 percent frequency of b is 0.65 percent. Next the standard error of the difference between the two proportions is calculated by the formula $SE_{diff} = \sqrt{SE_1^2 + SE_2^2}$. Substituting, we get $SE_{diff} = \sqrt{(.0069)^2 + (.0065)^2}$, which, simplified, yields $SE_{diff} = 0.95$ percent. Comparing this figure with the actual difference of 0.5 percent, we see that the actual difference equals 0.53 standard errors $\left(\frac{.5}{.95}\right)$. Last of all, referring to Table One, we see that an actual difference of 0.53 standard errors may be expected to occur on a basis of pure chance in 59.62 percent of all pairs of proportions. Since this percentage of probability is considerably larger than the 5.0 percent maximum probability which indicates a likelihood that some factor other than chance is in operation, we conclude that the present deviation is probably due merely to chance, and hence not significant of a real difference in frequency between the two phonemes in the dialect as a whole.

2.22 *Specific Uses of the Standard Error of the Difference.* The nature of the standard error of the difference between two proportions has been explained and illustrated so that it should be apparent that the device indicates accurately whether the actual difference found between two sampling proportions is significant or not. Any study of the relative frequency of the structural units of a language must provide some means of analyzing which differences in frequency found in a sample of linguistic forms are probably due to sheer chance and which are probably due to a real difference in the whole language or dialect being studied. The standard error of the difference between proportions, coupled with a reference to a probability chart such as Table One in the present study, provides a simple, concise method of indicating how likely it is that the difference between two proportions in a sample is significant. It would seem, therefore, that the standard error of the difference, or some similar statistical device, is definitely to be preferred, for the purpose mentioned, to a purely subjective comparison of two proportions.

When the investigator completes his count of, say, 1000 phonemes from a

dialect of American English, he may report the frequencies and indicate where the significant differences lie in some such form as the following:



Such a tabulation tells at a glance all the pertinent information of frequency.

One caution about the use of these statistical devices in the analysis of language should be noted here. The formulas, as has been previously indicated, were developed from empirical evidence in such matters as coin-tossing and dice-throwing. It is a characteristic of these matters, if the coin or dice are without bias, that no factor except the complex and little-understood series of causations known as *chance* influences the occurrence of the events studied. Any series of coin tosses or dice throws with unbiased coins or dice is a *random sample*, and a number of such samples always tend to show a *normal distribution* of deviations.

In language, however, since each formal unit is in some way bound up with its meaning, there is clearly an element other than chance influencing the occurrence of the events studied. It is more improbable than probable that a given series of linguistic forms from a single text will constitute a random sample. To consider an extreme case, it is obvious that the popular song from the period of World War I, 'K-K-K-Katy,' will contain more frequent occurrences of the consonants *k*, *g*, and *m* than will the language as a whole. As a linguistic text, such a song would certainly not constitute a random sample. Moreover, many less obvious factors of influence upon the frequency of linguistic forms may lie hidden in any given linguistic text. Poetry generally, with its conscious attention to sound, is suspect of not providing a random sample for purposes of quantitative linguistic analysis; and even differences in prose style may have some effect on quantitative linguistic data.

Application of the statistical techniques set forth above indicates to the investigator how likely it is that some factor other than chance has influenced the proportions in a sample. They cannot, however, indicate what factor has influenced the proportions. Such indication is the province of qualitative analysis. To pursue a practical working method, the quantitative linguistic investigator must collect his samples of evidence from a variety of sources within the area of the language that he is investigating. In this way, he trusts that those factors which may tend to prejudice the results of his study will cancel one another out. If sufficient care is taken to make a sample truly 'random,' significant analytical results can be obtained from it.

3. *The Role of Quantitative Linguistic Analysis.* Up to the present point, quantitative linguistic analysis has been defined and contrasted with qualitative linguistic analysis, and it has been demonstrated further that, as regards the questions (1) how much quantitative evidence should be collected for a reliable

analysis and (2) when may quantitative differences found in samples of evidence of linguistic frequency be considered significant, a statistical approach provides the most adequate methods of arriving at answers. Reserved for consideration at this point is a more basic question: What can quantitative analysis contribute to linguistic knowledge in general?

If one wishes to analyze a language either synchronically or diachronically, a purely qualitative analysis is incomplete. A qualitative synchronic study discloses the nature and variety of linguistic forms but in no way indicates the frequency or magnitude of such forms. In theory, at least, a form which occurs but once in a given language is entitled to as much descriptive emphasis in such a study as is a form which occurs in almost every utterance in the language. A synchronic description which presents a picture of a language entirely in terms of the nature and variety of its forms is like a cooking recipe which describes qualitatively the ingredients in a dish without indicating the proportions in which they are combined. The nature of the mixture is clearly indicated, however, if the language is analyzed quantitatively as well as qualitatively and if the results of the one type of study are presented along with the results of the other.

Diachronic descriptions of a language are properly built up by a comparison of synchronic descriptions of the same language at different historical periods. It is through qualitative diachronic descriptions of a single language and through the allied tool of the comparative method that the recognized rules of linguistic change such as sound change, analogical change, and borrowing have been discovered. Such rules of qualitative change, however, describe merely the manner of initiation of change and the overall correspondences between the language of different periods resulting from such changes; they do not attempt to describe the basic processes involved in the changes.

It appears probable that the process of any linguistic change, between the period of its initiation and the time when it has become established (if this occurs ultimately) throughout an entire language, is largely a matter of fluctuation in the frequency of linguistic forms. Such fluctuations in frequency, of course, require quantitative analysis if they are to be adequately described and understood. An adequate diachronic description of any phase of a language, therefore, must be based upon a comparison of both qualitative and quantitative synchronic descriptions of the language at different historical periods.

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INTONATION AND ANALYSIS

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Is English intonation susceptible of phonemic analysis? If not now, will it be so later? What kinship is there between tonal and phonemic events, and is there such a thing as a tonal segment? How successful have been the attempts thus far to subject English intonation to phonemic analysis?

These and related questions beset linguists as they turn their attention to the musical elements in language. The answers given to them, by the directive that they exercise upon coming linguists, have the power to further our understanding of intonation or to block it. As 'linguistic scientists' we know the dangers of extrapolation. Yet to those of us who are trained in a given technique, extrapolation of that technique is a constant temptation. The object of this paper is to consider the extension of current phonemics to intonation, from two points of view: (a) the resemblance, or lack of it, between the two fields, affecting the assumption that the same technique would work for both; (b) the success of sample attempts to extrapolate.

A. How closely do intonation and phonemics resemble each other?

1. *Are they alike in their backgrounds?* G. L. Trager, in his review of Pike's *Phonetics*, points out that all phonemicists are largely self-taught, that the science is barely out of its swaddling clothes.

In a sense this is true. The literature *on* phonemics is new. But, looked at another way, the literature *of* phonemics is very old. The common-sense knowledge of phonemics is as old as alphabetic writing—has been with us for several millennia. Where would the phonemicist have got his start without the impetus given by the prior study and comparison of these devices?

By contrast, the common-sense knowledge of intonation is virtually nil. Aside from the crude (and, where questions are concerned, inconsistent) signs of punctuation, there exists not even one system of representing English intonation that has been tested by widespread use. There is thus even less literature *of* intonation than *on* it, wretchedly meager as the latter may be. This would seem to suggest that our logical first step ought to be to supply some of the missing material, and postpone working on it as analysts until we have a respectable supply. New as phonemics is, there was a sizable literature of phonetics, observational and experimental, on which to base it. The study of intonation has very little such literature; what writings there are do not, for the most part, deal with specific questions in intonation, but with intonation as such—instead of being studies they are attempts at compendia. If we imitate them, and begin by systematizing the insufficient data now on hand, we can only expect to have our systems rendered obsolete as fast as each new datum comes to light.

2. *Are they alike by nature?* Let us examine points of contrast.

a. The segmental phoneme is semantically discontinuous. In pronouncing *mama* it makes no difference, within the limits of discrimination, whether the

initial *m* is made twice as long or half as long; twice as long will not result in 'more mama' and half as long will not result in 'less mama.' The signal, apart from the surprise that would be elicited by too great a departure from the norm, elicits the same response. By contrast, every intonational movement—with extremely infrequent exceptions—is continuous. If a note of warning consists in an upward movement of pitch, the intensity of the warning varies proportionately as the range increases or decreases.

b. The phoneme is arbitrary. Though here and there some vestiges of appropriateness remain, it makes no difference, by and large, whether the signal is *mama* or *papa*. The intonation curve, on the other hand, is tied at every point to the speaker's nervous tension. Intonation contains a few arbitrary uses, but they are embedded in a matrix of instinctive reactions; even the arbitrary uses may generally be assigned values consistent with the nervous interpretation (for instance, sentence fall occurs when the issue is settled, i.e. tension relaxes). The purely arbitrary uses of intonation are hardly more numerous than the expressive uses of the articulations of phonemics; they are opposite in the frequency of their respective kinds of use.

c. The phonemes comprise numerous variables, each of which depends upon a different and special function of the articulatory organs. Between some phonemes there are scarcely any resemblances at all, apart from the fact that all are used in speech; and where there are resemblances, as between *m* and *n*, the functional differences are usually easy to single out. Tone, on the other hand, is but one variable, which is smooth-running through its entire gamut; furthermore, the articulatory organs, with minor exceptions (as in the shift from chest tones to falsetto), are the same, and function in the same way, for the whole of intonation. If we made articulation the basis for determining phonemes, there would be only one pitch phoneme,[1] the functioning of the intonation-producing apparatus, i.e., intonation itself. As we shall observe, the 'pitch phonemes' that have been singled out do not owe their existence to objective differences between one and another, but to relative position on the pitch scale (and to the simple fact—a wholly subjective fact—that it is convenient to create divisions where none are objectively found). High pitch is meaningless out of relationship with lower pitch, for it must be separately determined for each speaker; it is true that in practise we can ignore such individual variations as between a tenor and a baritone; but we cannot ignore them as between a man, a woman, and a child. No such high degree of relativity affects the ordinary phonemes. The consequences of this are serious, for they suggest that the proper phonemes of pitch are to be found not in position, but in direction: not high versus low, but up versus down. This results in yet another contrast with the ordinary phonemes: it is as if we were unable to treat of *m* as an entity, but had to take as our fundamental unit the shift from *m* to something else.

[1] Or conceivably three, if below-singing-level, chest tones, and falsetto were distinguished. Such a functional distinction, however, has not to my knowledge been made. K. L. Pike seems to regard it as impractical: 'This characteristic [falsetto] is not subject to auditory articulation analysis.' *Phonetics* 128 (Ann Arbor 1943).

3. *Is segmentation practical in intonation studies?* Segmentation is not only practical, but necessary, if the varying features of pitch are to be talked about. The question then becomes, What kind of segmentation?

To one trained in the analysis of speech into bi-unique elements, it will be tempting to treat of pitches likewise as consisting of elements that can be lined up end to end. Pike[2] says, 'English uses four pitches. These four pitches are not absolute tones equivalent to precise musical notes. They are related to each other.' Wells[3] says more categorically, 'There are four pitch phonemes,' which he designates by Arabic numerals from *1* (lowest) to *4* (highest). While Pike recognizes the wavering of each individual pitch, there is nothing to indicate that he disagrees with Wells in regarding each pitch as a unit; the wavering is presumably a wavering about a norm, the norm being, as in conventional analysis, the phoneme, comprising allophones.[4]

What is the magic of the number four? As we have seen, there is nothing in the nature of one pitch that distinguishes it from another, in the manner in which *m* is distinguished from *n*. So the number might as well be six, or ten, or as many semitones as can be counted in the range of the voice. The division into four is arbitrary and for convenience only, and it would have been more scientific to avow it as such. There *could* exist, to be sure, a more objective rationale of the four: that it enables us to represent all the significant pitch morphemes; this claim is, in fact, made by Pike. But as we examine the morphemes cited by Wells and Harris (and to some extent by Pike)[5] and the remarks about them, we may readily see that there has been no such careful collecting and editing of morphemes as might determine how many pitches are needed. In fact, the whole problem has been approached backwards: instead of starting with abundant material, and deciding on the basis of the nature of that material what is to be done with it, the theoretical technique has been evolved in vacuo. For a mathematician or a logician this is orthodox procedure; for a linguist it seems out of place.

Harris uses seven numbers, 0 to 6. However, he treats sequences as his units, and the numbers are significant only as they are found in fixed sequences. These sequences are objective, and it may turn out that a serviceable intonational system can be built on them; until the painstaking collection of many, many examples confirms it, however, such a system remains pure theory—and mean-

[2] As co-author of *An Intensive Course in English*, 81 (Ann Arbor 1943). This article was written before the publication of Pike's *Intonation of American English*, Ann Arbor 1945. As I have criticized this book elsewhere (*Studies in Linguistics* 5.69–78) I do not interpolate references to it here. My criticism is essentially consistent, as Pike maintains his four-tone system.

[3] Rulon S. Wells, The Pitch Phonemes of English, *Language* 21.30 (1945).

[4] We are given no indication, by either musical or acoustic notation, of what is the approximate range of each pitch and whether all ranges are of equal extent. In one of Pike's examples (131) that of 'She notices it,' where *-ces* is assigned to 'low' and *it* to 'mid' pitch, I find in my own speech a difference of only four semitones.

[5] Zellig S. Harris, Simultaneous Components in Phonology, *Language* 20 (1944). K. L. Pike, *The Intonation of American English*, Ann Arbor 1945.

while we still need some kind of working segmentation which will enable us to record our morphemes.

Harris is not so fastidious as to object to overlapping segments. I believe that, for the time being at least, we must be free to use any serviceable device, whether or not it means mixing criteria and shifting the point of view. If we find, in physiology, that a man lives by exercise and also by eating, we should not be too proud, just because exercise and eating overlap and in other ways violate the niceties of subdivision, to mention them both in the same chapter.

Since I have set forth in another paper[6] such a working method of recording tonal morphemes, space will not be given to it here. Suffice it to say that the scheme is non-logistic, being an attempt merely to give a running record such as one has in written music.

B. How dependable are sample phonemic analyses of intonation?

Let us use, as an index to dependability, the accuracy of the statements about pitch morphemes. This is the safest basis of criticism, for such statements are subject to immediate and objective test.

First the statements made by Harris:[7]

1. "We find the relative pitch sequence 1221130 [where 3 = highest pitch and 0 = lowest] in . . . */We can't tell when they're coming/* . . . . Among utterances with the same stress positions we do not find other pitch sequences ending in 30." Using this same locution, and changing the uninhibited stress on *can't* to an inhibited one, we get the sequence 2111130, which upsets the rule.

2. "Occurrences of relative high pitch 4 at one or more places in such utterances [utterances ending in 30] will always be accompanied by a loud contrastive stress." But an unstressed *it* may occur with pitch 4, as in "It simply takes too many," with the sequence 4221130.

3. "As a result of such manipulations a large number of pitch sequences ending in 30 become identical." If the wish to get "identical" patterns overrides all consideration of semantics, this may be true; otherwise it is not.

4. "After a sequence of segments in which each loud-stressed segment is higher-pitched than the preceding, we never get a low-pitched segment: . . . in *Is your brother going?* we do not get a low-pitched *ing*." Using this same locution, with loud stress on *bro* and *go* (and also on *your*, if you like), one often hears a low-pitched *ing*. In fact, such a profile would naturally follow a statement like "How many times do I have to ask you?", showing that the question has been iterated.

5. "The 1221130 of *I don't know where he's going* . . . and the 2230 of *Bud Clark fumbled* are positional variants of the 230 in *He told him.*" It is doubtful that any contour which drops in pitch at some intermediate point is a mere positional variant of one which does not—unless, again, one ignore semantics.

Now for the assertions of Wells:[8]

1. "Under circumstances to be stated below (Semantic Values), a single vowel

[6] The Representation of Tonal Profiles.

[7] *Op. cit.*, 189 and 202–3, footnotes.

[8] *Op. cit.*, pp. 32–35.

may be affected by two pitch phonemes, but never by more than two." I quote this entire, though there is nothing in the later discussion of semantic values which seems to limit the assertion. What of the exclamation *oh!*, with an "I've caught you now" admonitory implication, beginning at low pitch, rising, and then falling? Or *well*, starting high, going down, and then rebounding—used when a speaker is about to concede a point but is still deliberating?

2. "Pitch phoneme *1* [lowest of Wells's four pitch phonemes] does not occur initially." This is an astonishing statement—if for no other reason, in view of the wide divergences among individuals. Mr. George Sanders, for example (as in his performance in "Hangover Square"), can express at times, and quite intelligibly, whole phrases below singing level. But profiles in which low pitch is initial are abundant: "Are you positive?" can have *are you* at a lower pitch than the average conclusive intonation.[9]

3. "*[3]He who [2]hesitates is [31]lost.* In *hesitates*, the syllables *-si-* and *-tates* are lower-pitched than *he-; -tates* in addition has a very slight rise at the end." But, keeping always within his contour 3231, we may have a slight upmotion after the downskip or a slight downmotion after it—with, what is more important, semantic differences.

4. "It seems likely that besides allophony, there is considerable free variation in phonemically identical pitch contours." Perceptible variation of any kind coupled with variation in meaning is a condition which in ordinary phonemics would frighten anyone away from labeling it as "free." The last examples cited above are "phonemically identical" in Wells's *1234* system, but differ in meaning. The contrast in meaning is even more marked in the "identical" "I can understand your resentment, but *I'm not to blame*," in which the pitch moves up rapidly on *I'm*, with a downglide to mid-pitch on *blame*, with *blame* held at a perceptually level tone or allowed to rise slightly at the end—identical by Wells' system since the slight terminal rise would not be sufficient to carry the pitch out of range *2*. But the level ending is admonitory, and the upmotion suggests reasonableness or pleading.

5. "The pitch morphemes are so few in number." This statement, better than any other, establishes the criticism that these discussions have simply not been preceded by a study of intonational facts.

6. "The morphemes into which pitch phonemes enter contain no other kind of phonemes." As a hypothesis, conditioned by the desire not to mix criteria, this is perhaps valid. But as an assertion, particularly in the discussion of "Semantic Values," it cannot have been derived from the direct observation of pitch morphemes. These cannot be abstracted from certain other elements than pitch, particularly stress and stress position.

7. "Elaborate discussion of the semantics of pitch phonemes (qua morphemes or constituents of morphemes) is outside the scope of this paper. It will suffice to note that pitch phoneme *4* is a morpheme in itself, with the meaning 'surprise'. As the final pitch phoneme in a sequence, it indicates surprised question-

[9] The depth of terminal descent is apt to be overestimated. The contrast between it and a preceding high pitch is so great that the fall is misjudged. This illustrates again that direction is, if anything, more important than position in intonation.

ing; when followed by *1*, it indicates a surprised assertion." This is the only unequivocal statement about meanings in the paper, and, small as the territory it covers, it is less than half right. Pitch *4* followed by pitch *1* (terminal) is not a "surprised assertion" if the stress comes *after* the highest pitch. Pitch *4* may occur as the final phoneme in a non-question. And even in a question, pitch *4* may be terminal without indicating surprise; in fact, a pitch even higher than *4* —the falsetto—may occur finally (after an upskip) in questions without the slightest implication of surprise, but rather with a nuance of sweetness.

One appreciates Coleman's words, as true now as they were thirty years ago, that "investigations hitherto have been deprived of much of their value through the inquirers' not going far enough afield (thus building up a theory on the few obvious examples that occur to one at the moment) or through their taking their examples from the connected language of books where the sentences are often altogether wanting in the variety found in conversational speech." For linguists who wish to be scientists there is only one scientific procedure: *gather the facts first*, then theorize about them. In intonation, the facts are morphemes, or profiles, that speakers are heard to use, together with the implications (determined by responses and otherwise) that these carry. To take as the starting point anything but such facts is to write with the mind open, perhaps, but with the ears closed. Until such a collocation of facts is at hand, we shall not even know whether a phonemic treatment is indicated, much less whether some particular phonemic system is valid. One who wishes to do effective work in intonation must realize that it, like any other field of scientific inquiry, is cultivated piecemeal by many searchers over many years; he must therefore not be contemptuous of applying himself to any one of the innumerable problems that block the way to a final solution. To attempt to lift bodily from another field the solution that was eventually achieved there, and apply it willynilly here, is to behave as if it were possible to get where we are not without starting from where we are.

I venture to list at random a few of the questions answers to which, both in themselves and in suggesting other questions and so gradually broadening our grasp of intonation, may some day make a precise, logical schematization possible. I hasten to disclaim any desire for "looseness" in the treatment of intonation; each problem must be attacked with parsimony of assumptions and with all the tightness of reasoning at our command; in short, scientifically.

1. What are the peculiarities of intonation of social groups? We have observed that preachers, circus barkers, and professors favor certain modes; what are these modes?

2. What are noteworthy idiosyncracies? Actors reveal these, as do ordinary folk.

3. Are idiosyncrasies genuinely original, or do they represent only a favoring of one kind of standard intonation over another, as when one person will say "so help me" and another "you betcha"?

4. What social occasions elicit particular intonations? A tie-up of this sort is one of the objective tests of the semantics of intonation.

5. What intonations are not used in English? One method of answering this

is to study the errors made by foreigners which make their speech seem un-English.

6. What intonations are favored with particular locutions? Though not especially in search of these myself, I have recorded them for *always, and then some, how I love that man, I wouldn't know, in the first place, silly boy!*, and others. It is hardly enougn for the student of intonation to content himself with a handful of oddities (the "Ripley" approach, to quote Bernard Bloch); the record must be rounded out. This means going beyond the whimseys of *yes, no, hello,* and *goodbye;* and in syntax it means investigating a few other things besides sentence-final intonations.

7. What is the place of intonation in communicative behavior as a whole? Bloomfield has referred to intonation as "gesture-like." This is a promising hint that deserves to be followed up.

8. What intonations do actors use when they simulate particular meanings or feelings?

9. How do profiles compare in frequency of use?

10. What is the relationship of specific profiles to position in reciprocal discourse (initiating, terminating, repeating, etc.)?

11. Are there intonations used by adults but not used or less used in the speech of young children who have progressed beyond the purely imitative stage and are composing their own sentences? The answer to this might make a beginning at separating the learned from the instinctive in intonation.

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NEUTRALIZATION IN AMERICAN ENGLISH VOWELS

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In a recent article in WORD, André Martinet found it quite understandable that English-speaking scholars 'feel so little tempted to make use of the concept of neutralization, whereas French and Russian linguists, whose mother tongues show such frequent instances of the situation which elicited the concept, usually make an extensive use of it.'[1]

Implicit in Martinet's statement is the conclusion that English is possessed of few, if any, instances of neutralization or suspension, so that the neglect of this concept by American linguists, for example, is a kind of linguistic ethnocentrism.

It is interesting to note, however, that there are not few, but many examples of neutralization in American dialects. The fact that these do not manifest themselves very often in morphological alterations may, to some extent, explain why they have generally been overlooked. And yet, pointing them out will probably contribute to a better understanding of 'a situation . . . which gives . . . trouble to the average student of phonetics and phonemics.'[2] The facts which I am about to present are not original with me. On the contrary, they are very well known; they have simply never been properly evaluated.

A number of the most striking instances follows. In all cases the capital letter represents the vowel archiphoneme which includes the range of phonetic values indicated. The dialect divisions are to be considered as only approximate. Abbreviations used for broad divisions are: GA = General American, S = Southern, E = Eastern. Eastern includes: NE = New England, MAC = Middle Atlantic coastal, NYC = New York City.

1. There is, to begin with, widespread neutralization of phonemic oppositions before *r* (including the [ə] which is often a positional variant for [r] in Southern, New England and New York City types of speech). We have, therefore, neither [ʊ] [ɛ] [ɪ] nor [u] [e] [i] in such words as *poor*, *mare*, and *spear*, but archiphonemes representing the suspension of these oppositions. The common practice of most American phonemicists is to identify the vowels in these positions with either one of these possibilities. Bloomfield, for instance, notes that a 'non-distinctive peculiarity of our diphthongs is their divergent sound, in most American types of pronunciation, before [r]: in this position they approach the character of a single long and rather tense vowel.' But he makes no analysis of the situation, and merely proceeds to transcribe such words as *gear*, *air*, *sure* with the 'long' vowels of *bee*, *bay*, and *do*.[3] Twaddell, although he sees the necessity for formu-

[1] André Martinet, About Structural Sketches, *Word* 5.13 (1949). About neutralization and archiphoneme, see, by the same author, *Phonology as Functional Phonetics*, 4 f, London, 1949.

[2] Cf. Kenneth L. Pike, *Phonemics* 141 B, Ann Arbor 1947.

[3] Leonard Bloomfield, *Language* 124–5, New York 1933.

lating adequate methodology to deal with this specific problem, fails to follow through on the basic premises.[4]

2. The reader will notice that the above examples are cases of vowel plus final *r*. Where *r* is intervocalic the situation is somewhat more complex. Here you can find instances of archiphonemes presenting even larger areas of suspension as it were. For example:

[ɛ] as in met
[æ] " " mat
[e] " " mate
} + r in most GA become E as in { merry, marry, Mary

As Morris Swadesh mentions in his 'On the Analysis of English Syllabics,' what I have transcribed as E varies between [ɛ] and [æ] in his own Chicago GA speech.[5] He describes this, however, as free variation, not as neutralization. It is of course both—phonetically the former, phonemically the latter.

This is similar to the French situation, where as Martinet points out, without neutralization we are 'faced with an insoluble problem when we want to transcribe the word *maison* in which standard speakers are apt to use any timbre ranging from [ɛ] to [e].[6]

In some GA and Southern dialects only [ɛ] and [e] fall together, so that *merry* and *Mary* are homophones; *marry* remains [mæri]. In most Eastern and Southern speech there is no neutralization of [ɛ] and [æ]. The status of the vowel in *Mary* in such dialects varies: in NYC speech it can be identified with the vowel of the noun *can*; in many Southern coastal dialects with [e].

A few American dialects carry the neutralization process very far. For instance, the sub-dialect of Brownsville, a district in Brooklyn, has:

bear *weary*
beer *wary*
} = [bɛə] [wɛəri]

Tendencies of this sort are found in other NYC sub-dialects and in Southern coastal and hill dialects.

3. [ɪ] *ill*
[i] *eel*
} + r in many GA *parlers* become I as in { *spirit*, *spear it*

Harking back again to section 1, the cases of neutralization there were those of vowel before final *r*. With intervocalic *r* the situation is more diverse. Many Eastern and Southern dialects distinguish carefully between *spirit* [spɪrɪt] and *spear* [spɪə(r)], *spear it* [spɪərɪt]; *mirror* [mɪrə(r)], *mere* [mɪə(r)], *merer* [mɪərə(r)]. But, in the many GA dialects mentioned above, neutralization is complete before *r*, whether final or intervocalic. The quality of the vowel may vary. In some dialects, it is closer to [ɪ]; in others it is closer to [i].

[4] W. Freeman Twaddell, On defining the Phoneme, *Language Monographs* 16.54 (1935).
[5] Morris Swadesh, On the Analysis of English Syllabics, *Language* 23.142 (1947).
[6] Martinet, About Structural Sketches, p. 20.

4. [ɔ] *law*, [o] *low* + r in some NE, many GA and all MAC dialects become O in *horse*, *hoarse*, *for*, *four*

In Southern, some General American, and many New England dialects, this neutralization does not take place.

5. [ɛ] *met*, [ɪ] *mitt* + n, m in many S dialects become E in *pen*, *hem*, *pin*, *him*

E here indicates vowel quality between [ɛ] and [ɪ]. Neutralization before *ŋ* is also reported in some Southern dialects, but I have no definitive information on this.

The instances I have set forth here constitute but a fraction of the numerous cases of neutralization in American dialects. There are, in addition to the vocalic examples, a number of consonantic neutralizations which I have not included in this communication.

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FAR FARE QUALCHE COSA

ROBERT L. POLITZER

The origin of the construction *faire faire une chose à quelqu'un* has always been an object of interest to Romance philologists. Only recently interest in the question of the origin of the expression was renewed by the publication of a study by Professor Dag Norberg.[1] In his study Norberg tried to prove that the construction can be explained by the extension of the dative normal in Latin after certain words of ordering (*mandare*) to other types of verbs (*iubere, facere*) and a similar extension in the use of the Latin active infinitive of purpose. Norberg's theory thus contradicted a previous theory advanced by Professor Henri F. Muller[2] who had explained the construction primarily by the confusion of the active and passive infinitive and a simultaneous extension of use of the Latin dative of agent. This confusion of the active and passive infinitive, according to Muller, was due initially to phonological reasons, namely the confusion of Latin ī with *e* in the final syllable, a confusion which made it impossible in the Vulgar Latin period to distinguish the active and passive infinitives of the first, second and fourth conjugations.

The weaknesses of Norberg's theory as opposed to Muller's were pointed out in a review by Professor Mario A. Pei in this journal.[3] They are primarily Norberg's inability to demonstrate that the active infinitive of purpose was ever a construction continuously employed in popular speech, and his admission that the confusion of active and passive infinitive could indeed have been a factor which aided in bringing about the construction. Yet, while pointing out the weaknesses of Norberg's demonstration, Pei himself raised what seems at first a formidable objection to the Muller hypothesis: Italian has a construction which completely parallels the French *faire faire* construction, although in Italian the final ī of Latin is always perfectly preserved. Thus the confusion of active and passive infinitive could in all probability not have taken place on Italian soil. As an explanation for the Italian construction and a way to save Muller's theory, Pei then advanced the suggestion that the Italian construction could have been imported from France after the Frankish conquest of the Langobard empire. At the same time, however, Pei admitted that evidence of confusion of the active and passive infinitive in Italian Vulgar Latin before the conquest by the Franks would invalidate this hypothesis.

What is then the evidence of the Vulgar Latin material from Italy on this particular question? Schiaparelli's *Codice Diplomatico Langobardo*[4] furnishes particularly useful material. The *Codice* contains by definition only documents

[1] Dag Norberg, "Faire faire quelque chose à quelqu'un," Recherches sur l'origine latine de la construction romane, *Språkvetenskapliga Sällskapets Förhandlinger*, Uppsala 1943–1945, Bilaga E, pp. 65–106.

[2] Henri F. Muller, *Origine et histoire de la preposition, 'à' dans les locutions du type de 'faire faire quelque chose à quelqu'un'*, Poitiers 1912.

[3] *Word* 2.90–2 (1946).

[4] Luigi Schiaparelli, *Codice diplomatico langobardo*, Rome, Vol. I 1929, Vol. II 1933.

written before the Frankish conquest. Furthermore, for the purpose of this investigation, only original documents (written between 720 and 774) from the central Italian area were considered. These documents indicate that the confusion of the active and passive infinitive did indeed occur in the Italian Vulgar Latin of the period, and we find in a document from Lucca (document 194) *me dominus de hoc seculo . . . vocare iussere* (if God should order me to be called—*vocare!*—from this world); in document 128 from Lucca, *ipsa cartula . . . fecimus . . . exemplare* (we had this charter copied); other examples of this type of confusion are: *si nus . . . dominus . . . conservare iusseret* (No. 138, Lucca); *pagina firmare . . . previdimus* (No. 127, Lucca); *demandavet cartula relevare . . . per ipso notario* (No. 113, Lucca); *per hanc cartulam firmare te provideo* (No. 273, Lucca); *me rectorem ordinare iuberis; ut mihi dimesuriam dare iuberis* (both from No. 272, Lucca). The reverse confusion, namely *i* for *e* in the active infinitive, occurs sporadically: *donari et offeri deberemus* (No. 91, Lucca); *ab homine defensari non potuaerimus* (No. 80, Lucca).

What are we to conclude from this evidence? At first this may seem to be the blow against the Muller theory which Pei had visualized. However, one obvious thing must be pointed out: the confusion seems to occur only with infinitives in which it could have arisen for phonological reasons only. If the use of the active for the passive infinitives in the examples quoted above was due to syntactical reasons, namely the survival of the infinitive of purpose, then we should certainly expect also numerous examples of third conjugation infinitives in the construction. The evidence thus seems to indicate the apparently impossible, namely that the construction originated in Italy independently from France, but precisely for the reason adduced by Muller.

A brief consideration of the phonological pattern of the Vulgar Latin of the Eighth Century and of the morphological factors involved in the question of the final *ī* may illucidate the problem. From the evidence of the Vulgar Latin documents as well as the subsequent development in the Romance languages, it becomes clear that at that time Vulgar Latin had the following vowel phonemes: *a e ɛ o ɔ u i*. In unaccented position, the opposition of *e* to *ɛ* and *o* to *ɔ* was neutralized, and in the atonic penult this neutralization was carried even further to a single vowel phoneme.[5] The important question for the problem under consideration is of course the phonemic pattern of the vowels in the final syllable.

[5] In Vulgar Latin documents of the Eighth Century the vowels *a i ɔ u* nearly always appear with correct classical Latin orthography in accented syllable. Only *e* (from classical Latin *ĕ* and *ī*) and *o* (from classical Latin *ŭ* and *ŏ*) are rendered by the orthography *i* or *e*, and *u* or *o*, respectively. In the unaccented syllable, also *ɛ* and *ɔ* are drawn into this orthographic confusion. Cf. M. A. Pei, *The Language of the Eighth Century Texts in Northern France* 14–62 (New York 1932); R. L. Politzer, *The Language of the Eighth Century Lombardic Documents*, New York 1949, Part I, A; Frieda N. Politzer, *Dialectal Features in Eighth Century Vulgar Latin Documents of Italy*, Columbia dissertation, approved 1949, unpublished, Part I, par. 1 and 2. The latter work, which is based on a study of the *Codice Diplomatico Langobardo*, also shows examples which attest complete neutralization in the atonic penult: *latare* for *latere*, *latura* for *latera*, *fibila* for *fibula*, in Eighth Century central Italian documents.

Here we must first consider the evidence of the texts. In addition to the phenomena attesting the neutralization of *e*/ɛ and *o*/ɔ, we find the confusion of the fourth with the second declension,[6] a phenomenon which will indicate that the *u* phoneme no longer existed in the final syllable. With regard to the front vowel, we find the already mentioned confusions of the active and passive infinitives, and sporadically also other substitutions of *e* for *ī*, as for instance in the genitive of the second declension, *monusculi nostre* (No. 67, Lucca) or in the plural of the second declension, *futures* (No. 127, Lucca), *de ipsi diacones* (No. 93, Pisa). In quoting these examples it must of course be admitted that they, just as the very frequent substitutions of *e* for *ī* in the ablative adjectives or in the dative of the third declension, can be accounted for by morphological confusions. Thus from the evidence of the documents we can reconstruct the following phonemic pattern for the vocalic system in the final syllable: (*u*) *o a e i*. This picture is indeed also the one that corresponds to the present state of affairs in Central Italian. The essential question of course is whether the confusion of *ī* and *e* attested in the documents corresponds to an actual phonological confusion in the final syllable during the formative period of Italian, and whether the stability of Latin final *ī* in present day Italian perhaps does not tell us the whole story of the historical development.

From the phonemic point of view the following considerations may be advanced in favor of the theory that an actual confusion between the *e* and *i* sounds was possible also in the central Italian area: the tendency to neutralize the close–open opposition must have been particularly strong in the final syllable where the weakening of the vowel must have been considerable. In addition the disappearance of the *u* in the final syllable had left an asymmetrical partial vocalic system, and the pressure to reduce to a simple triangular system was thus present.[7] The tendency to throw out the *i* sound from the final syllable corresponded thus to an actual necessity in the phonemic system. The confusion attested in the documents thus corresponds to the lack of the *i* in the final syllable in the phonemic pattern, if not of central Italian speech of the Eighth Century generally speaking, at least of the speech pattern of some individuals.[8] The loss of *i* in the final syllable in the speech pattern of parts of the population could easily account for the emergence of the *faire faire* type of construction.

[6] Examples for the absorption of the fourth declension type *manus*, *manūs* by the second declension *populus*, *populi* are attested already in the classical period. Cf. Édouard Bourciez, *Éléments de linguistique romane*, Paris, 4th ed. 1946, par. 94a.

[7] To speak here of a further neutralization of the degree of aperture opposition in the final syllable encounters some difficulties: since *e* and *i* are members of a gradual opposition in which the latter presents the extreme of the feature, namely minimum aperture, *i* and not *e* should be the result of a genuine neutralization. Cf. N. S. Trubetzkoy, *Grundzüge der Phonologie* 74 (Prague 1939; French 1949 edition not available).

[8] There is of course no reason to assume identity of phonemic patterns between individuals from the same area. See André Martinet, *La Prononciation du français contemporain*, Paris 1945, for a study of the often quite divergent phonemic patterns of the French of individuals from the same geographic area. There is no reason why we should not attribute at least the same lack of homogeneity to Eighth Century Romance speech.

The question which must finally be asked is why the loss of *i* in the final syllable was repressed in Italian and did not become a feature of the language, as for instance in Spanish, which emerges with a simple triangular vowel system in the final syllable. The answer seems to be that in central Italian the tendency to symmetrize the system by the loss of the *i* sound was counteracted by the necessity to preserve that sound as an essential morpheme. As long as the final *-s* still existed in the language, it could take the place of the *-ī* morpheme, the essential function of which is of course to denote the plural of the noun and the second person singular of the verb. As a matter of fact, with some very minor exceptions,[9] only those Romance languages which have lost the final *-s* have kept two front vowel phonemes in the final syllable. Thus we may conclude that in the central Italian area the potential reduction of *-ī* in the final syllable was counteracted by the fall of final *-s*, a development which seems to have occurred in the central Italian area at precisely the time from which the documentary evidence here used has been taken.[10]

The comparison of Vulgar Latin documents from France and Italy with respect to the question of the preservation of final *ī* shows, as one might expect, that in France the confusion of *ī* and *e* was by far more frequent.[11] Yet the difference is a statistical, not an absolute one. Thus it seems that in the question of the preservation of vowels in the final syllable the Romance languages in their divergence merely carried out potentialities which to lesser or greater degrees were present in the phonological patterns of the Vulgar Latin speech throughout the Romance world. Thus, it does not seem warranted to the writer to have recourse to an explanation of linguistic borrowing in the case of the *far fare* construction of Italian, and the explanation for the origin of the construction which was advanced by Muller thus still remains the one which seems most acceptable.

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[9] Portuguese preserved some remnants of final *ī* in the past tense until the Fourteenth Century. Cf. *Grundriss der romanischen Philologie*, Gustav Gröber ed., Vol. I, Strassburg 1888, p. 743. The one example of a Romance language which has kept two front vowel phonemes in the final syllable as well as the final *-s* is the Logudorese dialect of Sardinia. This dialect, however, has kept two back vowel phonemes in final syllable as well. Cf. Pei, *The Italian Language* 151 (New York, 1941).

[10] This is clearly demonstrated in the unpublished doctoral dissertation of Frieda N. Politzer (see footnote 5), which reveals that the fall of final *-s* progressed from south to north in Italy during the Eighth Century. This is shown by a study of the relative distribution of the fall of final *-s* and connected morphological phenomena in the documents originating in five areas distributed from central to northern Italy.

[11] Pei, *The Language of the Eighth Century Texts in Northern France*, p. 49 and Appendix p. 369, finds 110 examples of change of final *ī* to *e* in the eleven documents of his first group; of these, however, 47 examples which occur in the ablative adjective (cf. *op. cit.*, p. 379) must be deducted to give a figure comparable to the one found in the dissertation of F. N. Politzer, where this change in the third declension singular is not included in the discussion of the phonetic development. At any rate, only 16 examples of the change are found in 173 documents from the central Italian area.

TONES IN THE RIMING SYSTEM OF THE SUI LANGUAGE

FANG-KUEI LI

In the course of studying Sui, a language spoken in the southeastern part of the Kweichow province, China, a number of folk songs were gathered. In these songs rimes appear in great abundance, and this offers us an opportunity to study a special factor which seems to play a part in the riming system, namely the tones. For the riming syllables are regulated not only by the vowel or diphthong and the final consonant but apparently also by the tone.

There are six tones in the Sui language. They are 1) a low level or low rising tone, 2) a low falling tone, 3) a middle level tone, 4) a high falling tone, 5) a high rising tone, and 6) a high level tone.[1] While there is a strong tendency to rime syllables of the same tone, there are sufficient cases to show that syllables of different tones may rime. It is the purpose of this paper to show what tones are permitted to rime and to discover what tones show the closest affinity to each other.

A few preliminary remarks are necessary in regard to the general form of these songs and the position of the rimes. Of the 2600 lines of songs gathered by me, a great majority (roughly 2000) consists of seven syllables in a line with a cesura (pause) after the third syllable. This may be considered the normal line. A small percentage, however, consists of eight syllables in a line with a cesura after the fourth syllable, and another small percentage has eleven or twelve syllables in a line with two cesuras, one after the third or the fourth syllable and one after the seventh or eighth syllable. There are also occasional lines of six, nine, thirteen, or fourteen syllables, but they are extremely rare. The main types can be shown by the following diagrams, using a dash to represent a syllable:

1) — — — — — — —
2) — — — — — — — —
3) — — — — — — — — — — —
4) — — — — — — — — — — — —

The position of the rimes that occur in these lines is partly fixed and partly variable. According to whether the rimes occur within a line or in two adjacent lines, the rimes may be divided into two types, the internal rime and the external rime. In internal riming, the first member of the riming pair occurs on the syllable immediately before the cesura and the second member falls on any of the following syllables in the line (types 1 and 2) except the last. In lines with two cesuras (types 3 and 4) two internal riming pairs may be found. The first member of the first riming pair occurs on the syllable before the first cesura and the second member on any of the following syllables except the last one before the second cesura; and the first member of the second riming pair occurs on the

[1] Cf. Language 24.160–7.

syllable before the second cesura and the second member on any of the following syllables except the last. In external riming, the first member of a riming pair falls on the last syllable of the first line, and the second member on any syllable in the second line except the last. Thus, while the first member of a riming pair, internal or external, occupies a definite position in the line, the second member is rather free in position. Not all lines in our collection show rimes, but over ninety percent of them show either internal rimes or external rimes or both. Roughly two thirds of them show internal rimes and about an equal number of instances show external rimes.

The following selection from a song about the creation of the world is given, with a free translation, as an illustration of the type of poetry with its rimes.[2] The internal rimes are in italics and the external rimes are marked with asterisk.

γan^{1} sjen^{3} *tša*^{3}	tsi^{5} ni^{4} *ŋa*^{3} *toŋ^{2}
γan^{1} ʔăi^{1} **wo*ŋ^{2}	q'ăm^{3} *toŋ*^{2} ho^{4} *mja^{1}
tsi^{5} q'ăm^{3} **ŋa*^{3}	kon^{3} peŋ^{5} *da*^{1} wăn^{1}
peŋ^{5} ni^{4} ʔjət^{5}	tun^{1} pu^{3} me^{2} *ʔňi^{6}
peŋ^{5} ni^{4} *ňi^{6}	tun^{1} pu^{3} ʔnăŋ^{1} *ʔňam^{1}
peŋ^{5} ni^{4} *ham^{1}	me^{2} ʔŋo^{3} to^{2} *ʔdi^{5}
peŋ^{5} ni^{4} **sɪ*^{5}	ja^{6} pu^{3} *mi*^{4} *ʔŋo^{3}
peŋ^{5} ni^{4} **ŋo*^{4}	kwi^{2} *mo*^{4} bjaŋ^{3} *da^{1}
peŋ^{5} ni^{4} ljok^{4}	ɱu^{5} *ɱa^{1} sum^{3} tsən^{2}
peŋ^{5} ni^{4} *šət*^{5}	wo^{5} *γət*^{5} sum^{3} n̥ăi^{1}
peŋ^{5} ni^{4} *pat*^{5}	fum^{3} ʔma^{1} *qat*^{5} *šu^{1}
peŋ^{5} ni^{4} **tšu*^{3}	fai^{4} *nu*^{4} qan^{5} qoŋ^{1}
peŋ^{5} ni^{4} sup^{4}	fum^{3} măi^{4} tšau^{1} *djət^{5}
ni^{4} sup^{4} **ʔjət*^{5}	fum^{3} *šit*^{5} tšau^{1} *ja^{1}
dau^{1} tu^{3} sai^{3}	me^{2} peŋ^{5} *ha^{1} lju^{4}
saŋ^{2} ʔdau^{3} *ni*^{4}	tsăp^{4} *li*^{4} tsăp^{4} *kaŋ*^{1} tsăp^{4} *faŋ*^{1} tsăp^{4} jin^{2}

Translation:

The supernatural being made a bronze bow.
The king held the bronze bow in his hand.
The bow and arrow were made capable of shooting the suns.
When the first sun was shot down, the heat was not diminished.
When the second sun was shot down, the heat was still terrible.
When the third sun was shot down, there was no sign of weakening.
When the fourth sun was shot down, there was also no abatement.
When the fifth sun was shot down, buffaloes and cows opened their eyes.
When the sixth sun was shot down, pigs and dogs then arose.
When the seventh sun was shot down, eels and loaches wiggled.
When the eighth sun was shot down, green mustard plants grew.

[2] Notes on the symbols used: *q*, *q'* are uvulars; *k*, *ŋ*, *γ* etc. are velars; *š*, *tš*, *ň*, etc. are palatals, *ʔn*, *ʔm*, *ʔň*, *ʔŋ*, *ʔj*, *ʔd*, etc. are preglottalized consonants; *n̥*, *ɱ̥*, etc. are voiceless nasals; *w* and *j* are the semivowels; *e* and *o* are open; *a* is front, low; *ə* is back and slightly raised. Tones are marked by raised numerals.

When the ninth sun was shot down, brothers and sisters worked.
When the tenth sun was shot down, trees and shrubs of firewood grew.
When the eleventh sun was shot down, reeds and grass grew.
We deliberated together, and stopped shooting.
Only one sun was left; every mile was bright, and every region was tranquil.

It can be seen from the preceding selection that in some cases the external rime coalesces with the internal rime, if the second member of the external rime happens to fall on the syllable immediately before the cesura, as in lines 2, 3, 7, 8, etc. These syllables, which participate both in the internal and the external rimes, are indicated by asterisk and italics.

It can also be seen from this selection that tones seem to play a part in riming, namely there is a strong tendency to rime syllables of the same tone. This is not strange as we have a similar phenomenon in Chinese poetry. On the other hand there are also cases of rimes of different tones. To find out whether there is any tendency for any particular tones to rime together, the first three hundred lines of our collection were taken as a sample and the rimes that occur in the lines are counted. There are altogether 417 rime pairs, and they are distributed according to the tones as in the following tables:

Riming with	1	2	3	4	5	6	Total
Tone 1	110	47	7	3	2	3	172
Tone 2	47	54	3	1	1	0	106
Tone 3	7	3	64	37	1	0	112
Tone 4	3	1	37	14	3	1	59
Tone 5	2	1	1	3	34	22	63
Tone 6	3	0	0	1	22	10	36
Percentages (those below ten percent are omitted):							
Tone 1	64%	27%	—	—	—	—	91%
Tone 2	44%	51%	—	—	—	—	95%
Tone 3	—	—	57%	33%	—	—	90%
Tone 4	—	—	63%	24%	—	—	87%
Tone 5	—	—	—	—	54%	35%	89%
Tone 6	—	—	—	—	61%	28%	89%

It is sufficiently clear from the preceding tables that while there is a strong tendency to rime syllables of the same tone, there is an almost equally strong tendency to rime tone 2 with tone 1, tone 4 with tone 3, and tone 6 with tone 5.[3] It seems, as far as the riming system is concerned, that we may classify the tones into three groups each of which consists of two tones. Syllables belonging to one group of tones are permitted to rime freely with each other, but rarely with syllables having tones belonging to other groups. As our collection consists of pure folk songs, the occasional irregularity in riming is easily understandable.

[3] Syllables ending in *-p*, *-t*, *-k* are not included in the preceding tables of rimes, because they are capable of having only tone 4 and tone 5 and can show no other preference. In the following discussion the syllables ending in *-p*, *-t*, *-k* are excluded.

These groups of tones are interesting, because there seems to be no special phonetic reason for them to be so grouped. Tone 1, a low level or low rising tone, is grouped with tone 2, a low falling tone; tone 3, a middle level tone, is grouped with tone 4, a high falling tone; and tone 5, a high rising tone, is grouped with tone 6, a high level tone. An explanation may be needed, but such an explanation is no more than a theory supported by these very facts and confirmed by some other considerations.

It is interesting to observe that these groupings seem to have some correlation with another method of grouping of the tones as suggested in a previous article of mine (Language 24.165–7). There we divide the tones into two groups according to the initials which may be found with them. We further suggest that tones 1, 3, and 5 which form one group are developed chiefly from tones with voiceless initials and that tones 2, 4, and 6 which form the other group from tones with voiced initials. As the distribution of tones and the initials in Sui show sporadic exceptions, we conclude that loans and secondary developments may have caused these disturbances. The six tones can be assumed to have developed from an original three tone system, later differentiated into the modern six tones according to the initials, but it is not possible to determine from the distribution of the tones and the initials which two tones should be combined as representatives of an original tone. Now the riming system indicates that there are three pairs of tones, each pair being permitted to rime freely, and that in each pair there is one tone from the group supposed to have developed from a voiceless initial and another tone from a voiced initial. Since no satisfactory phonetic reason can be given for the tones to be so paired, this system must represent a tradition going back to the time when the original three tones were not yet differentiated according to the initials. If so, these three pairs would represent the original tones, and the system is a tradition fairly faithfully kept.

This is confirmed by a study of the older type of loan words from Chinese. The three Ancient Chinese tone categories 'Even', 'Rising', and 'Departing' are represented in Sui respectively by tones 1 and 2, 3 and 4, and 5 and 6 according to the initials. Thus, each pair of tones in the riming system corresponds to one Ancient Chinese tone. A few examples will suffice to illustrate the development of Chinese tones into Sui, the Ancient Chinese forms being given according to Karlgren's reconstruction:[4]

		ANC. CH.	SUI
Even:	Voiceless initials	tung 'east'	toŋ[1]
		tieng 'nail'	tjeŋ[1]
		kwa 'melon'	kwa[1]

[4] The Ancient Chinese is the language of the 6th century as represented by the dictionary *Ch'ieh-yün*. Karlgren's reconstructions are essentially the same as given in his *Analytic Dictionary of Chinese and Sino-Japanese*, Paris 1923, with some slight modifications as given in his *Grammata Serica*, Stockholm 1940. *Even*, *rising* and *departing* are simply names given to these tones in the 6th century; a fourth, the *entering* tone, is given to syllables ending in *-p*, *-t*, and *-k*.

		ACN. CH.	SUI
	Voiced initials	d'ung 'same'	toŋ[2]
		d'âng 'hall'	taŋ[2]
		jịwang 'king'	woŋ[2]
Rising:	Voiceless initials	təng 'steelyard'	tăŋ[3]
		pâu 'protect'	pau[3]
		xậi 'sea'	hai[3]
	Voiced initials	d'iem 'mat'	tjem[4]
		lji 'mile'	li[4]
		ngwa 'tile'	ŋwa[4]
Departing:	Voiceless initials	t'ậm 'inquire'	t'âm[5]
		t'ân 'charcoal'	t'ân[5]
		puân 'half'	pan[5]
	Voiced initials	d'i 'earth'	ti[6]
		d'iei ordinal	ti[6]
		mịwɐng 'fate'	miŋ[6]

These loans explain not only why tones 1 and 2, 3 and 4, and 5 and 6 rime together, but indicate also that tones 1, 3, 5, and 2, 4, 6 are developed from voiceless and voiced initials respectively.

There is another layer of loans which are late and in which the correspondences of tones are quite different. The following examples will illustrate this. In these loans the Modern Chinese forms will also be given, and will be quoted in the Northern Mandarin (Peiping) dialect according to the Wade-Giles romanization for the convenience of reference. It can be seen that the development of tones in these loans is very similar, as a system, to the Northern Mandarin, although the actual contours of these tones are very close to the actual tones of the local Southwestern Mandarin rather than to those of the Northern Mandarin.

	ANC. CH.	MAND.	SUI
Even: Voiceless initials	tâu 'knife'	tao[1]	tau[3]
	sịän-ka "an immortal"	hsien[1]-chia[1]	sjen[3]-tša[3]
Voiced initials	d'âng 'hall'	t'ang[2]	t'aŋ[4]
	d'ung 'same'	t'ung[2]	t'uŋ[4]
Rising: Voiceless initials	kâm 'dare'	kan[3]	kan[6]
	hâu 'good'	hao[3]	hau[6]
Voiced initials (liquid, nasal, etc.)	ngâ 'I'	wo[3]	ŋo[6]
	lji 'a surname'	li[3]	li[6]
Voiced initials (stops, affricates, fricatives, etc.)	d'iei 'younger brother'	ti[4]	ti[1]
	dz'âu 'to make'	tsao[4]	tsau[1]
Departing: Voiceless initials	tâu 'to reach'	tao[4]	tau[1]
	k'jẹi 'air'	ch'i[4]	tš'i[1]
Voiced initials	d'i 'earth'	ti[4]	ti[1]
	man 'slow'	man[4]	man[1]

It is interesting to observe that some of these loan words exist in two forms, one from the earlier layer and the other from the later layer, such as *toŋ*^{2} and *t'uŋ*^{4}, *taŋ*^{2} and *t'aŋ*^{4}, *ti*^{6} and *ti*^{1}, etc. The later loans differ little from the local Chinese dialect so that they must have been borrowed quite recently, the earlier loans show quite different developments.

In our songs both the earlier loans and the later loans appear, so that these songs can not be very old. On the other hand the riming system of the tones seems to point back to an old tradition. I have not been able to find any manuscript of these songs and it is doubtful whether they have ever been written down, although a slightly modified system of writing apparently derived from Chinese is in use in books on divination. These books are, however, only read by the few shamans who are specially trained. It seems then that the system of riming must be due to an oral tradition. In Chinese poetry if one frequently rimes tone 1 with tone 2, we may suspect that the tradition is kept up by the large number of rime books which always classify the words according to the ancient tones, but there are certainly no rime books in the Sui language.

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MOHAVE VOICE AND SPEECH MANNERISMS

GEORGE DEVEREUX

There is an extreme paucity of data pertaining to the management of the voice in non-Indo-European languages. Hence it may be of some interest to describe the vocal behavior of the Mohave Indians during conversations dealing with sexual topics. Since the Mohave discuss sexual matters, especially those not pertaining to the process of gestation, rather freely—being, in this respect, as in many others, different from their non-Yuman neighbors (13)[1]—the following data also shed some light on the overall vocal behavior of the Mohave. A detailed discussion of Mohave phonetics is not necessary in the present context, since this problem is adequately covered by Kroeber's monograph (12).

The Mohave are a talkative people, who admire skilled orators, witty conversationalists (4) and expert swearers (9), though they have no liking for people who are merely coarse or verbally sadistic (3). Their enjoyment of talk is so great that one frequently finds oneself following with great interest the conversation between one's informant and interpreter, without being able to understand more than one word in a hundred, simply because they give the impression of being wholly absorbed by, and involved in, the act of communication.

It is, hence, not surprising that elaborate precautions should be taken to prevent the birth of mute children, by means of a meticulous compliance with the relevant taboos (6). Yet, the ability to speak is not thought of as a criterion of intelligence, since even fetuses about to be born, as well as twin infants and nursing babies are believed to be capable of understanding, and responding to, rational verbal admonitions (3), even though they are manifestly incapable of speech.

The Mohave of both sexes have pleasing voices, which they manage with consummate skill. They exploit to the utmost the musical and expressive potentialities inherent in a language possessing pitch, and transfer even to English some of the musical qualities of Mohave speech, without approximating the sing-song impression produced by Swedes.

Despite their skilful management of the voice, the Mohave do not seem to be *consciously* interested in sound-production. Thus, the date at which the male puberty rite occurs is not determined by the cracking of the adolescent's voice (8). Furthermore, unlike the kindred Yuma (11) the Mohave deny that the laughter of women differs from that of men, except in its pitch, and perhaps also its volume.

It is also striking that, despite the intensity of the Mohave Indian's participation in the act of verbal communication, extreme forms of voice-production are rare or absent. In other words the Mohave are seldom shrill and do not whisper frequently. It is also significant that I have never met, or heard of, an individual possessing several 'voices', although in Western society certain latently homosexual and grandiose individuals frequently have three distinct 'voices':

[1] Parenthetic numbers refer to the attached bibliography.

a basso boom, a shrill scream and an aggressive, low-pitched whisper. The avoidance of extremes of voice-production is especially conspicuous when a Mohave man quotes and imitates the speech of a woman or of a transvestite: The narrator does not change to falsetto but uses his normal voice, suggesting, rather than mimicking, nuances of female speech, even in imitating a male transvestite's use of the female expletive *peleley* (2).

Imperfections of Voice-Production and of Speech are obstacles to communication which are sometimes rooted in neurotic mechanisms and represent, among other things, an aggression toward, or a repudiation of, one's interlocutor (10).

(a) *Whispering.* Although the Mohave may whisper for reasons of discretion (6), I have never observed the type of whispering well known to psychiatrists, which is at once a reaction formation against the urge to scream at one's interlocutor, and a hostile act, which defeats the listener's desire to understand what the speaker *allegedly* 'intends' to tell him.

(b) *Disregard for the Hearing Range* is relatively frequent among the Mohave, who sometimes continue to talk even after their erstwhile interlocutor has passed beyond the effective hearing range. This mannerism appears to be due to inertia, rather than to unconscious aggressions (7).

(c) *Mumbling.* Mohave informants, conversing with the interpreter about matters of which they are not quite certain, sometimes cease to speak in a manner which obviously subsumes an audience, and engage in a sort of musing pseudo-monologue, marked by a blurring of pronunciation, and sometimes also by a lowering of the pitch of the voice and a slowing down of the normal tempo of speech. The mumbler's behavior gives one the impression that he is re-testing and re-appraising his knowledge by means of verbalization: He utters his sentences hesitantly, and seems to listen to his own voice, as though he were thinking "This doesn't sound quite right" or else "This does sound right, doesn't it?" I have never heard an informant use mumbling in a hostile or resistive manner.

(d) *Rhythm and Tempo.* The Mohave have a traditional staccatto, strongly accented and rather rapid manner of delivering traditional memorized texts which are usually couched in brief sentences (1, 13). This characteristic method of delivery is so completely a part of the recited text, that it is very difficult, even for the most willing informant, to slow down to the point where the text can be conveniently recorded. Although adhesions between a traditional text and its usual manner of delivery are, of course, to be expected in a non-literate society, they can be exploited for the purpose of resisting communication. Thus, when working with the rather timid and not overly willing old Nyahwera, in the presence of a shaman whom he feared, my interpreter and I found it impossible to induce him to utter slowly the archaic text (1, 13) of the 'recitativo' songs of the initiation ritual for transvestites (2). On the other hand my friends and willing informants Hivsu Tupoma and Ahma Humare always managed, after a few false starts, to dictate native texts at a speed which made it possible for me to record their recitations verbatim.

(e) *Text of Songs.* Everyone is familiar with the difficulty of reciting the text of a song without singing, at the same time, the musical part thereof. This diffi-

culty is so genuine that, when Ahma Humare taught me his weylak curing songs, I had to stop him from time to time to record certain half-heard words. In order to do so, I had to pronounce first what I *believed* the word to be, in order to enable him to repeat it after me in the *correct* manner. Likewise, after recording an entire sentence, and after reading it back to him, Ahma Humare was usually able to repeat this sentence without singing it because, for the first time in his life, he had heard it recited without music. This observation underscores the significance of auditory factors in learning, in a non-literate society. The adhesion of the text to the music does not appear to be exploited for the purpose of inhibiting communication because of unconscious resistances and aggressions.

(f) *Pseudo-Communication* does not, properly speaking, belong in a study of voice-production, and is, hence, mentioned only because it also exemplifies defective communication. The shaman T.B., reputed to be a witch, managed to confuse completely both my very competent interpreter and myself. Not merely did he skip from topic to topic, not merely did he contradict himself periodically but even went so far as to utter what seemed to be long series of non-sequiturs. On re-reading the recorded conversation I am forcibly reminded of the ramblings of a moderately well-preserved schizophrenic, although there is no reason whatsoever to suspect T.B. of being a schizophrenic, or of having been in a dream-state induced by a conversation about the shamanistic and esoteric aspects of mental disease, as seen from the shamanistic point of view, since—significantly enough—his voice-production and speech mannerisms were not in the least unusual or deviant. In the end both my interpreter and myself independently reached the conclusion that T.B. was deliberately confusing us.

(g) *Suppression of Means of Communication.* The Mohave show a singular reluctance to speak English, unless they speak English well. On the other hand they do not seem to pretend not to understand it when, in reality, they do happen to know a little English. Thus old Mrs. Chach who knew only scraps of English, always made a genuine effort to understand what I said to her, or in her presence,—even to the point of laughing out loud even before some affectionate jibe of mine (4) was interpreted to her—although, except on one occasion, she never attempted to utter a single English word. In 1938, however, when I visited her to bid her goodbye before leaving the reservation, after asking the interpreter to translate several of her affectionate parting remarks to me, she took my hands and with great warmth bade me 'Good-bye.' The formal English words were pronounced very hesitatingly, clearly indicating that she barely knew them. She probably used them only because she admittedly had a premonition that she would not see me again, and, hence, presumably wanted to speak to me directly at least once in her life.[2] The shaman Hivsu Tupoma, one of my closest friends and most willing informants, readily admitted that he knew a few words of English, but insisted on using an interpreter. On one occasion, however, when, under the influence of alcohol he decided to speak to me of his acts of witchcraft,

[2] Significantly enough, the same (correct) premonition also caused her to violate the taboo on being photographed. After my departure she asked a young Mohave to take her picture and to send it to me, so that I would not forget her (4).

he saw me in private, and thus revealed that he spoke broken, though intelligible English (5). I know of no English-speaking Mohave who, because of his contempt for Whites (4, 5) or for some other reason, pretends not to speak that language, although e.g. Pueblo Indians are known to use this mode of evasion.

(h) *Temporary Mutism.* The Mohave Indian, who is not addicted to sulking (11), does not refuse to communicate by simply assuming an attitude of stolid silence, or by playing dumb, which is the traditional defense of less proud and less mercurial minority groups. On the other hand, although all young people speak English fairly well, shy youngsters, and especially young schoolgirls, sometimes become almost speechless with embarrassment. A little genuine friendliness generally suffices, however, to induce them to converse in a cheerful, easy and animated manner.[3]

The Expression of Emotions is a notoriously elusive matter. The following observations may, hence, be of some interest.

(a) *Merriment:* When discussing scabrous matters, or when engaged in affectionately obscene banter (4), speech tends to speed up and to alternate with guffaws, though never with giggles, snickers and salacious whispers. The transition from speech to guffaw is a swift, though a gradual one, and many Mohave almost, though not quite, manage to laugh and to speak at the same time. The face becomes relaxed and the features expand into a mobile and infectious smile.

(b) *Embarrassment:* On rare occasions, especially when I had just begun to work with my octogenarian friend, Mrs. Chach, I was able to detect a certain amount of embarrassment in her speech. Once or twice she even clapped her hand before her mouth, a gesture also known to be used by embarrassed men (13). During one of our early interviews she repeatedly pronounced the word *modha·r* (penis) as *mo·dha·r*, greatly exaggerating the length of both vowels. When I naively inquired whether this was an archaic or idiosyncratic manner of pronouncing the word in question, both Chach and my interpreter laughed and explained that the old lady was merely a trifle self-conscious. Her embarrassment also manifested itself in a slightly greater meticulousness in reproducing the regular pitches and stresses of ordinary pronunciation. This peculiarity of speech disappeared very rapidly, and, later on, when we exchanged affectionate insults Mrs. Chach gave me rapid-fire tit for tat, without the least trace of embarrassment (4). On a subsequent field-trip, on hearing her pronounced *hispan* (vagina) as *hišpan*, I asked whether she again felt embarrassed. Both Chach and my interpreter guffawed and told me that her pronunciation was due solely to the absence of upper incisors. She even put her right index in her mouth to show me the gap in her teeth.

(c) *Solemnity and Reserve.* When discussing the serious side of sexuality, i.e. gestation (6), my informants' voices tended to become more resonant, and were a trifle lower pitched than usual. Pronunciation also tended to be unusually

[3] According to Róheim the kindred Yuma are sometimes referred to by local whites as vertable 'sphinxes' (14). This does not jibe with my own impressions of the Yuma, nor have I ever heard a white inhabitant of Parker, Arizona, describe the Mohave as stolid, silent or mysterious.

meticulous. At the same time they displayed a tendency to use short, clipped, 'preaching-style' sentences, and the characteristic staccatto delivery of orators, or of persons narrating a major myth, which has already been described above. When speaking in this manner their faces bore a frown of concentrated attention.

Summary. The Mohave are expert speakers and conversationalists, who manage their voices with consummate skill and tasteful restraint. Gross abnormalities of speech-production and obstacles to communication were absent.

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NOTES DE GRAMMAIRE ET D'ÉTYMOLOGIE ÉTHIOPIENNE

WOLF LESLAU

1. Quantité vocalique à valeur phonologique.

Contrairement à ce qui se passe dans les autres langues sémitiques, la quantité vocalique n'a pas de valeur phonologique en éthiopien. En fait il n'y a presque pas de cas en éthiopien où une voyelle brève existe à côté d'une voyelle longue ou inversement.[1] Il n'en est pas de même en harari et en tigré où il y a opposition entre voyelles longues et brèves.

Pour le harari j'ai trouvé une opposition entre:

ū: u, dans *qūl* "bâton": *qul* "chose grillée";

ā: a, dans *ḥāl* "mauvaise habitude" (probablement de l'arabe *ḥāl*): *ḥal* "il est";

ī:i, dans *zīq* "partie de vêtement": *ziq* "fort"; *fīz* "chaton" (probablement de l'arabe *faṣṣ, fuṣṣ*): *fiz* "droit"; *išīš* "ne touche pas!" (interjection): *išiš* "beurre"; *-īt* est la marque du féminin (*gidīr:gidirīt* "grand", *qeḥ:qeḥīt* "rouge"): *-it* est un démonstratif (*yi bāqālit* "ce mulet-ci").

J'ai mentionné ici des groupes de mots différenciés seulement par la quantité vocalique. Par ailleurs, l'opposition entre *a i u* long et bref est courante dans la langue, comme p.ex. *būq bāya* "fermenter": *buf bāya* "bouder"; *gidīr* "grand": *ṭiḥín* "moulu", etc.

En Tigré aussi l'opposition *a:ā* a une valeur phonologique. Ainsi *hab* "donne": *hāb* "chaleur"; *'afa* "regarder": *'āfa* "guérir"; *'agna* "plier": *'āgana* "plier plusieurs fois"; *ḥal* "tante maternelle" (probablement de **ḥalt*): *ḥāl* "oncle maternel". Quant à la voyelle *u* elle est toujours longue dans le participe passif *qětūl*, et brève dans d'autres cas.[2]

Il y a, de ce phénomène, deux explications possibles. Ou bien cette valeur phonologique est due à l'influence de l'arabe, langue parlée par la presque totalité des habitants de Harar, et en très grande partie par les habitants des régions parlant le tigré. L'arabe est précisément une langue où la quantité vocalique joue un grand rôle. Ou bien c'est une conservation d'un état ancien de l'éthiopien. En effet le guèze, pour autant qu'on puisse en juger par la prononciation traditionnelle,[3] avait des voyelles longues et brèves, et surtout *a* (représentant la voyelle du 1er ordre du guèze) et *ā* (representant la voyelle du 4ème ordre). Le problème de la quantité des voyelles *u i* resterait encore à examiner.

2. La sonorisation de *t* final en argobba et en ennemor.

La sonorisation de *t* final en argobba (*bed* "maisson" < *bet*) a été reconnue par

[1] Pour le guèze voir plus loin.

[2] Voir mes Supplementary observations on Tigré grammar, dans *JAOS* 68.127-39 (1948).

[3] Ainsi d'après E. Littmann, *Geez-Studien* (1917), passim; M. Cohen, La prononciation traditionnelle du guèze, *JA* 1921.217-68 en ce qui concerne la quantité des voyelles du 1ère et du 4ème ordre. Toutefois Mittwoch, *Die traditionelle Aussprache des Aethiopischen* 14 (1926) indique que la différence entre les différentes voyelles du guèze est qualitative et non pas quantitative.

M. Cohen d'après les documents receuillis par Mme de Monfreid[4] pour l'argobba-sud,[5] et d'après la notation de Abba Tasfa Sellasye[6] en écriture éthiopienne pour l'argobba-nord.[7] J'ai observé le même phénomène dans le dialecte gouragué de Ennemor, dialecte très proche du tchaha. Comme j'ai eu l'occasion d'enquêter également sur l'argobba-nord je voudrais apporter ici quelques éclaircissements à ce sujet.

D'une manière générale on peut dire qu'en argobba il ne s'agit pas d'une véritable sonore *d*, mais d'un *d* assourdi [d^{t}] ou peut-être d'un *t* adouci [t^{d}]. En ce qui concerne l'ennemor j'ai l'impression qu'il s'agit plutôt d'une sonore *d* presque intacte. Il serait souhaitable de pouvoir examiner simultanément les deux dialectes.

Voici la situation en argobba. D'une manière générale, *t* est sonorisé dans les noms et dans quelques formes verbales. Ce son sera transcrit ici *d*, mais en réalité il s'agit de [d^{t}].

Noms: *ɛ̈sad* "feu", amh.[8] *ɛ̈sat*; *hɛ̈rbad* "repas du soir", amh. *ɛ̈rat; ɛ̈hɛ̈d* "soeur", amh. *ɛ̈t*; *ɛ̈šed* "épi vert", amh. *ɛ̈šet*; *angäd* "cou", amh. *angät*; *çähed* "soleil", muher *çet*, guèze *ṣäḥay*; *wäd* "champ", gafat aussi *wäd*, č. *wat*;[9] *bed* "maison", amh. *bet*; *fid* "figure", amh. *fit*; *šɛ̈bäd* "cheveux gris", amh. *šɛ̈bät*.

J'ai également noté des exemples avec *t* final: *wɛ̈gat* "point de côté", *gulbät* "genou", *ɛ̈nšɛ̈rarit* "araignée", *ṭut* "sein", *lemat* "panier à pain", et les noms de nombre: *ket* "deux", *sost* "trois", *arbit* "quatre", etc.

Après consonne *t* semble préservé: *ɛ̈nbɛ̈rt* "nombril", *kist* "mensonge".

Dans le verbe, le *-ät* final de la 3ème pers. fem.sg. est également sonorisé: *säddäbäd* "elle a offensé", *säbbäräd* "elle a cassé".

Entre deux voyelles *t* est preserve dans *zämmätä* "piller", *mekkäta* "parer avec le bouclier", et dans quelques autres verbes trilitères. Le phonème *t* est sonorisé dans *moda* "mourir", et dans les formes verbales, comme *al-säkkärä-d-u* "elle ne s'est pas enivré", et dans le gérondif simple *ṭägbɛ̈do* "étant rassasié", et le gérondif composé *ṭägbɛ̈dul*.[10] Le *t* est aussi sonorisé dans les verbes à 3ème radicale *r* où, par suite de constitution syllabique due à la liquide *r*, le son *t* ne se trouve plus entre voyelles, comme dans *säkɛ̈rdo*, *säkɛ̈rdul*.

Dans le dialecte gouragué d'ennemor la sonorisation du *t* final semble être plus générale qu'en argobba. En ce qui concerne le son, j'ai fait remarquer plus haut qu'il s'agit véritablement d'un *d* sonore.

En position finale: *anäbäd* "langue", čaha *anäbät*; *angäd* "cou", amh. *angät*; *ɛ̈nid* "bambou", č. *ɛ̈net*; *bunyäd* "pigeon", č. *bunyät*; *qurind* "sommet", č. *qurit*;

[4] *Nouvelles études d'éthiopien méridional* 382-3.

[5] Argobba-sud est parlé dans le sud du Harar.

[6] *Etudes d'éthiopien méridional* 367.

[7] Argobba-nord est parlé dans la région d'Ankober.

[8] Amh. = amharique, č. = tchaha.

[9] Voir ici §4.

[10] On notera que le gérondif a un *t*, même dans les verbes 1.2.3. alors qu'en amharique le *t* du gérondif se trouve seulement dans les verbes du type *sämma* (originairement 3ème rad. laryngale) et *qärrä* (originairement une 3ème rad. *w y*).

amand[11] "belle-mère", č. *amat*; *bĕrăd* "fer", č. amh. *bĕrăt*; *anačwĕd* "tante", č. *anačwĕt*; *a^{w}ăd* "soleil", č. *eat*; *tu'năd* "cruauté", č. *tunăt* (rac. *tb'*).

Le *t* final est préservé dans *ĕsat* "feu"; *azgat* "ceinture en toile", č. *azgart*; *märxăt* "signe", č. id; *gĕt* "moitié", č. *gĕbt*; *arăqet* "sangsue", č. id., et quelques autres.

En position intervocalique, *t* est également sonorisé: *xuda* "lui", č. *xuta*; *betădă* "être large", č. *betătăm*; *čodă* "travailler", č. *čotăm*; *modă* "mourir", č. *motăm*; *sadă* "errer", č. *satăm*; *semandă* "uriner", č. *sematăm*; *xuwăda* "bouse de vache", č. *xuwăta*.

La sonorisation du *t* final a été signalée par E. Cerulli pour le somali.[12]

3. La voyelle finale de la 3ème masc. sg. du verbe tigre, harari et argobba.

En traitant de la voyelle finale de la 3ème masc. sg. du verbe tigré, j'ai pris parti pour la voyelle *ă* (donc *fägrä* "sortir")[13] plutôt que pour la voyelle *a* contrairement à Kolmodin[14] et à l'écriture éthiopienne adoptée par Littmann pour les textes tigré.[15] J'ai eu l'occasion de réexaminer le tigré à Keren, Erythrée, et le résultat de mon enquête est que la voyelle finale est *a* et non pas *ă*, donc *fägra*. Kolmodin a bien observé que cette voyelle n'est pas un *a* pur, mais plutôt un "dumpfes" *a*. Je voudrais ajouter que la voyelle après la 1ère radicale du verbe au parfait est également *a*, de sorte qu'on trouve *fägra* à côté de *fagra*, la qualité du 1er *a* étant la même que celle du *a* final.

Kolmodin explique l'existence du *a* final en tigré par analogie avec le *a* final des verbes à 3ème radicale *w y* (*fäta*) et il dit: "Da nun bekanntlich sowohl die Formen der 3 pers. pl. als auch der Suffix-formen der 3 pers.sg.masc. des starken Verbum nach Analogie der Verba ultimae *y* umgebildet worden sind, liegt die Annahme nahe, dass auch die suffixlose Form der 3 pers.sg.masc. in diese Analogie miteingezogen worden ist und also *qätla*, *qätläw* und *qätläyu* sich ebenso verhält wie *fäta* zu *fätäw* und *fätäyu*".[16] Cette analogie pourrait peut-être expliquer la forme du parfait en tigré où en effet la forme de la 3ème pl. *-äw* semble être une forme analogique des verbes à 3ème rad. *w y*, mais cette explication est rendue douteuse par le fait que le harari et l'argobba ont aussi le parfait 3ème sg.masc. avec un *-a* final: harari *säbära* "casser", argobba *käddäna* "couvrir". L'analogie des verbes à 3ème radicale *w y* ne peut pas expliquer la forme du verbe 1.2.3. au parfait de ces deux langues puisque nulle part ailleurs on ne voit les verbes à 3ème radicale *w y* exercer une influence sur les verbes du type 1.2.3. en harari et en argobba. La question reste donc à l'étude.

[11] A remarquer la nasalisation du *d* dans certaines racines contenant *r m*, mais ce fait n'est pas général.

[12] *Comptes rendus du GLECS* 4.56 (1947).

[13] The Verb in Tigre, *JAOS* 65.3 (1945).

[14] Über die 3. pers. masc. sg. perf. in Tigré, *Le Monde Oriental* 6.1–4 (1912).

[15] *Publications of the Princeton Expedition to Abyssinia*.

[16] Pour la même formation analogique dans certains dialectes arabes, voir Brockelmann, *Grundriss* 574, et Noeldeke, *Beiträge zur semitischen Sprachwissenschaft* 18–9.

Je voudrais signaler qu'en harari, comme en tigré, la voyelle du 1er ordre est souvent "obscure" et la distinction entre *ă* et *a* n'est pas toujours très nette. De même qu'en tigré, on voit en harari paraître une voyelle "obscure" *a* là où on s'attendrait à un *ă*. Comme dans le cas de la quantité vocalique à valeur phonologique (voir §1), on peut se demander si on ne doit pas attribuer à l'influence de l'arabe le passage de la voyelle *ă* à un *a* "obscur". En arabe, en effet, la voyelle "obscure" *a* est plus courante que la voyelle *ă*.

4. Une racine verbale secondaire en gouragué.

La racine čaha *čonam* "s'asseoir" a été considéreé avec raison par Polotsky[17] comme une racine verbale secondaire issue du thème réfléchi d'une racine *wn'* (*'* désignant une laryngale quelconque).[18] On pourrait y ajouter une autre racine de la même formation secondaire, a savoir čaha *čotăm* "bêcher, labourer, travailler", ennemor *čodă*, muher *čătăm*, maskan *čotă*. Dans ce verbe, le *č* initial est sans doute secondaire, issu d'un réfléchi *t*, si on compare la racine avec čaha, muher *wat* "champ", argobba, gafat *wad'*.[19]

La prépalatalisation d'un *t* primitif en *č* dans *čotă* cadre bien avec toute la classe de verbes à *o* après la 1ère radicale, en čaha. En effet, dans presque tous les exemples la 1ère radicale est prépalatalisée dans cette classe. Ainsi *ç̌orăm* "porter" (mais amh. *ţorä*), *žorăm* "tourner autour" (mais amh. *zorä*), *ǰosăm* "frapper avec la main".[20] A l'imparfait, la prépalatalisation reste: *yəç̌or*, *yəžor*, *yəǰos*, mais non pas au jussif: *yăţor*, *yăzor*, *yădos*. Il y a deux exceptions dans cette classe: *ţomăm* "jeûner", sans prépalatalisation du *ţ*, est probablement emprunté à l'amharique; et *šomăm* "nommer chef", imparfait *yəšom*, jussif *yəšum* (avec préservation du *š* au jussif et alternance *o:u* à l'imparfait:jussif) est probablement aussi emprunté à l'amharique.

5. L'article en gafat.

Il y a en gafat[21] un élément de détermination *-š* suffixé au nom: *kokob-ĕ-š* "l'étoile". Dans mes *Gafat Documents* (p. 43), j'ai identifié cet élément avec le pronom suffixe de la 3ème pers. masc. sg. en comparant les faits analogues de

[17] *Bulletin de la Société de Linguistique* 39.158 (1938).

[18] Aymallal a, en fait, *tonnam* avec *t;* la racine **wn'* se trouve, en dehors du gouragué, dans le gafat *aonnă* "déposer, placer."

[19] Est-ce que *wat* "champ" serait à rapprocher de Sidamo *wit* "semer," *wiţă* "semencee"? La racime *wiţ* du Sidamo semble toutefois être une forme secondaire de *miţ* (Cerulli, *Studi etiopici. II. La lingua e la storia dei Sidamo* 215, 224).

[20] On peut donc se demander s'il faut invoquer l'analogie du thème intensif pour expliquer l'alternance palatale: non-palatale comme l'a suggéré Polotsky, *op. cit.* 159.

[21] Le gafat est une langue sud-éthiopienne. Le seul document écrit de cette langue est la traduction du Cantique des Cantiques faite vers la fin du 18ème siecle. C'est sur la base de ce document que j'ai élaboré une esquisse grammaticale de cette langue dans mes *Gafat Documents, Records of a South Ethiopic language*, New Haven 1945. La langue a été considerée comme morte et comme remplacée complètement par l'amharique. Pendant mon séjour au Godjam au mois de mars-avril 1947, j'ai retrouvé quelques informateurs de la région du Nil Bleu qui parlent encore le gafat.

l'amharique et du harari.[22] Je me suis basé, pour cette explication, sur l'existence d'un pronom suffixe de la 3ème pers. masc. sg. *-š* en gafat. Or, mes recherches récentes sur le gafat m'ont fourni le pronom suffixe de la 3ème pers. masc. sg. *-(ĕ)ho*, et non pas *-š*. Mon explication de l'élément de détermination *-š* n'est donc plus justifiable. Je crois à présent que cet élément *-š* est à identifier avec l'élément d'insistance et de mise en relief *-s* "quant à, certes" connu par les langues éthiopiennes. La prépalatalisation de *s* en *š* s'expliquerait ou bien par un ancien **-si* qui existe en effet en tigrigna,[23] ou bien par une tendance générale à la prépalatalisation dont on trouve d'autres traces en gafat, comme *ahuñ* "maintenant" (amharique *ahun*), *bĕčĕlä* "mulet" (racine *bql*), *ĕǧǧä* "un" (amh. *and*) et autres, où la raison de la prépalatalisation n'est pas évidente au premier abord.[24] Il est important de signaler qu'un élément *-š* se trouve aussi en čaha (gouragué) avec une valeur de mise en relief: *säbu-š* "c'est un homme" (et non pas une femme), *xäda-š* "s'il te plaît", *name-š* "donne-moi donc".

L'élément *-š* du gafat est donc un élément de mise en relief et de référence. Cet élément est d'un emploi beaucoup plus fréquent que celui de l'article des autres langues éthiopiennes. En effet, en gafat, même un mot isolé est cité avec cet élément *-š* alors que dans les autres langues éthiopiennes l'article est employé surtout comme élément de référence et désigne un objet déjà mentionné.

C'est la valeur de mise en relief de cet élément *-š* qui expliquerait aussi son emploi avec un adverbe: *yahuněš* "maintenant-certes"; avec un verbe: *ěñäto bäyhoněš* "si ceci n'arrive pas certes".[25]

Quant au pronom suffixe *-š* de mes *Gafat Documents*, c'est une traduction de l'amharique *-u* dans sa valeur d'article, et non pas du pronom suffixe de la 3ème personne.

6. Amharique *mäksäño* "mardi".

Tous les éthiopisants ont reconnu que *mäksäño* "mardi" contient l'élément *säño* "lundi". C'est surtout l'élément *mäk*, *mak* qui demande une explication.

Ludolf[26] cite *magsaño*, et le met en relation avec un non-existant *mag* "le lendemain".

Praetorius[27] explique *mäksäño* comme provenant de *ka'ab säño* "deuxième lundi" avec passage du *b* en *m* et métathèse du *k* et *m*. On ne voit pas la raison pour laquelle "mardi" s'appelerait "deuxième lundi"; l'explication phonétique n'est pas probante non plus.

Armbruster, *Amharic-English Vocabulary* 206, compare *mak* avec Bedja *mah* "matin", Egyptian *meḥ* "to gleam, blaze". Mais *ib.* 249 il identifie *mak* avec *magĕst* "lendemain".

[22] L'article *-u* de l'amharique et *-zo* du harari sont à l'origine des pronoms suffixes. La situation est la même pour l'argobba *-u* et le čaha *-äta*.

[23] W. Leslau, *Documents Tigrigna* 149.

[24] Dans d'autres cas, la prépalatalisation dans la racine est due au voisinage de *i* ou de *y*: *bäššä* "pleurer", racine *bky*; *dängä* "pierre", amh. *dängiya*, etc.

[25] *Gafat Documents* 43.

[26] *Lexicon Amharico-Latinum* 18.

[27] *Die amharische Sprache* 27.

C'est en effet par *magĕst* qu'on peut expliquer l'élément *mak*. La terminaison *t* de *magĕst* est celle du féminin (sans avoir la valeur du féminin), et le mot serait *magĕs*. L'amharique *mäksäño*, *maksäño* proviendrait de *magĕs-säño* avec simplification du *s: mag-säño*, et assimilation d'assourdissement *gs* > *ks*. Le mot signifierait donc "le lendemain du lundi". Une expression analogue pour "mardi" se trouve aussi dans quelques dialectes gouragué: čaha *wuṭät märäga*, muher, aymallal *wĕṭät männag*, m-à-m. "lundi-lendemain", "le lendemain du lundi"; cp. aussi tigré *täla šäno* "qui suit le lundi". Un problème qui se pose est de savoir pour quelle raison "mardi" n'a pas son propre nom à lui, mais est exprimé par rapport au lundi.

Les autres langues éthiopiennes désignent "mardi" par la racine "trois, troisième jour": guèze *sälus*, harari *sälasa*. Le gafat a *mäksäñä* comme l'amharique ("lundi" étant *säñä* en gafat). Les dialectes gouragué du selti, ennemor ont *mägär gäbäya* qui signifie "le grand marché", mardi étant probablement le jour du marché hebdomadaire dans ces régions. L'argobba exprime "mardi" par *nur aḥusin* qui est un nom propre d'un saint[27a] arabe.

7. Le nom de nombre "neuf" en éthiopien méridional.

Il y a, en apparence, deux racines différentes pour le nom de nombre "neuf" en éthiopien: une racine *ts'* connue par ailleurs en sémitique et qu'on trouve en éthiopien septentrional: guèze *tĕs'u*, tigre *sĕ'* (mais *tasĕ'* "neuvième"), tigrigna *tĕš'atte*;[28] et une autre racine en éthiopien méridional: amharique *zäṭäñ*, argobba *zäḥṭäñ*, harari *ziḥṭäñ*, *zäṭä*, gafat *zäṭäññä*.

Reinisch[29] a comparé la racine sud-éthiopienne avec la racine **zagaḍ* "neuf" des langues couchitiques. Praetorius,[30] en se basant sur la forme *zäḥṭäñ* de Ludolf —forme confirmée par d'autres langues sud-éthiopiennes—l'a expliquée comme provenant de *z-ḥṭ-ñ* où *z* serait le relatif,[31] *ñ* le suffixe, et la racine serait donc *ḥṭṭ*, *ḥṣṣ* "être peu nombreux"; *ḥĕṭäñ* serait donc "das Geringere, Wenigere". Ni le sens, ni la morphologie ne justifie cette explication.

Il me semble que *zḥṭ* (*ñ* est sans doute un suffixe) pourrait s'expliquer par la racine sémitique *ts'* en passant peut-être par les étapes suivantes: *ts'* > *ṭs'*, *ṭsḥ* (par emphatisation de *t* en *ṭ* sous l'influence du ' et assourdissement du ' en *ḥ* par assimilation à la sourde *s*) > *sḥṭ* (par métathèse) > *zḥṭ* (par dissimilation d'assourdissement) et *zṭ* dans les langues qui n'ont pas de laryngales (amharique, čaha, gafat).[32] Je ne prétends pas que la langue a passé par toutes ces étapes

[27a] Voir M. Cohen, *Nouvelles études d'éthiopien méridional* 359; Cerulli, *Studi etiopici* 2 f. 7–8.

[28] Pour *š* du tigrigna, voir N. V. Yushmanov, A sibilant anomaly in the Tigriña numerals, *Akademia Nauk, Institut Yazyka i myshleniya. Africana* 1.77–87 (1937).

[29] *Das Zahlwort "vier" und "neun" in den chamito-semitischen Sprachen* 13.

[30] *Die amharische Sprache* 203.

[31] *z* comme relatif n'est employé dans aucune langue éthiopienne méridionale à l'exception du harari.

[32] Pour l'emphatisation du *t* sous l'influence du ' cp. tunisien *arba'ṭaš* "quatorze", *sba'ṭaš* "dix-sept", pour *arba'taš*, *sba'taš* (voir Brockelmann, *Grundriss* 241). Pour l'assourdissement du ' final en *ḥ*, voir Leslau, *Lexique Soqoṭri* 19.

successivement pour passer de *ts'* à *zḥṭ*, pas plus qu'un enfant prononçant l'anglais *everator* "ascenseur" au lieu de *elevator* ne passe par les étapes *elevator*, *evelator*, *everator*. Mais ces étapes pourraient expliquer l'aboutissement final. On pourrait imaginer d'autres étapes, mais le résultat serait le même.

Quelle est la raison de la transformation de la racine *ts'* en *zḥṭ*, *zṭ*? Elle n'est pas due uniquement à des raisons d'ordre phonétique, puisque la racine *ts'* s'est maintenue en éthiopien septentrional. Il faudrait probablement chercher la raison de la transformation dans le fait que le nombre "neuf" a une valeur mystique et sacrée.[33] Dans le monde sémitique on pourrait comparer l'arabe du Maroc et de Tlemcen où, par étymologie populaire, la racine *ts'* "neuf" devient *tĕs'ud*, *tĕs'ŏd* de la racine *s'd* "être heureux".[34]

Asia Institute, New York

[33] Voir *Encyclopedia of Religion and Ethics* 9.406 passim; F. E. Enders, *Die Zahl in Mystik und Glauben der Kultur-Voelker* 131–9 (1935).

[34] Brockelmann, *Grundriss* 296; W. Marçais, *Le dialecte arabe de Tlemcen* 156; Marçais, *Le dialecte arabe des Ulād Brahīm de Saïda* 144, explique que *tes'od* "neuf" paraît dans les dialectes ou *s'a* signifie "mendier", et pour cette raison (*t*)*s'* est transformé en *s'd*.

GEORGE KINGSLEY ZIPF, *Human Behavior and the Principle of Least Effort*, Addison-Wesley Press, Cambridge, Mass., 1949. x + 573 pp.

Speech being one aspect of human behavior, and Zipf being counted among linguists, the reader may expect that linguistic preoccupations will not be absent from this volume. As a matter of fact, speech is here so much in the foreground that the author finds it necessary at times to warn us that the 'underlying aim' of the book 'is not to present speech data,' but 'to demonstrate the universal validity of the Principle of Least Effort.' Linguistic facts, in statistical form, are used extensively, particularly in Part One, which is by far the larger of the two. But they are obviously meant to illustrate the working of Least Effort and to support the author's all-embracing hypothesis.

In a sense, the title of the book, by specifically mentioning human behavior, does not do full justice to its contents: though man is seldom out of sight, all living process is included in the demonstration. But, of course, few people would be tempted to doubt the validity of Zipf's assumption in reference to animals and plants if it were proven in the case of human beings. One of the main difficulties of the author's undertaking lies in the widespread reluctance to conceive of man's behavior as regulated in its last details by an all-governing principle which leaves no room for any free-will. The idea that Least Effort, in contradistinction to Line of Least Resistance, implies foresight, is not likely to reconcile those who refuse to be machines, even if they be calculating ones.

Our own objection to Zipf's views does not arise from a reluctance to accept the unexpressed theoretical consequences of his thesis, but from what appears, to this reviewer, an unwarranted narrowing of man's motives. It seems clear that minimizing of effort cannot be the ultimate goal of human or animal activity, which would be self-contradictory since activity implies effort. That goal is the satisfaction of needs, and it is in the pursuit of these needs that it may be argued that animate beings will tend toward 'the least average probable rate of work' which is the way Zipf defines Least Effort. Now, what if the satisfaction of one of these needs precisely consists in an expenditure of effort, so that the larger the expenditure, the more efficiently the need will be satisfied? Or should we define 'work' and 'effort' in such a way as to exclude from these concepts all activity, which the acting subject feels to be an end in itself? But how could we tell what is work and what is non-work? Should we say that work is what is minimized and non-work what is not? But then Zipf's book would become a huge tautology. We should have to consider that the same action, that of chopping wood for instance, is effort in the case of the farmer who might run the risk of freezing to death, and non-effort in that of the ex-Kaiser for whom it probably was nothing but a pastime from which he did not intend to derive any material or moral advantage. When a mountaineer selects a path of pronounced obstacles, Zipf construes his choice as determined by a desire to impress others, whereby he is likely to improve his social rating. Many, and this reviewer for one, would think

that the inner satisfaction of having surmounted obstacles is, in not a few cases, conducive to this course of action. And yet, in most cases, this inner satisfaction will inextricably combine with some amount of social ambition, so that we should be at a loss to evaluate the amount of work and that of non-work. As a matter of fact, we don't find in Zipf's work any attempt at such an evaluation. The problem of activity as the satisfaction of a need is not even mentioned. By applying his statistical approach to all available data in the various domains he chooses in order to illustrate his views, he finally shows that, *on the whole and in the long run*, economy appears as the main factor that determines the evolution of human behavior patterns. This is undoubtedly a precious finding, and we must be thankful to Zipf for his comprehensive demonstration of the tremendous importance of economy in the most varied fields of human behavior. The fact that this was widely suspected, and had previously been assumed as a working hypothesis in specific fields, does not make his contribution less impressive. And yet the book might have been more convincing had the author refrained from presenting Least Effort as an exclusive principle and also from giving vent to his political bias and personal grievances.

Most linguists will, at the same time, welcome a book, confessedly non-linguistic, in which speech data are aptly utilized in order to illustrate human behavior generally, and will regret that it brings so little that is really new in their own field. From the author of the Psychobiology of Language, many of us expected decisive contributions to our research. It is obvious that Zipf has been bent upon enlarging his field instead of concentrating upon his earlier findings. Since he does not even mention the name of Trubetzkoy or the title of the *Grundzüge der Phonologie*, it is difficult to ascertain whether he is aware of the fact that several European linguists hailed him as a pioneer in the domain of diachronic phonemics. On the whole it is rather disappointing to find that Zipf does not go very far toward interpreting the statistical speech data which he presents. The book under review unfortunately confirms the impression that, as a linguist, he is brilliant, but superficial, capable of presenting impressive generalizations on the basis of wholesale counts, but not inclined to check them against the intricate reality of linguistic experience. He seems willing to leave to others—Trubetzkoy, Kurylowicz, Haudricourt—the task of probing his views, improving and enlarging upon them, and frequently correcting them. Whether he agrees with these developments, we cannot tell, as no mention is made of them anywhere. He does not seem aware either of the existence of Roman Jakobson's *Kindersprache*, or of the considerations regarding phonemic economy presented by this reviewer in various publications, not to speak of a number of European contributions to phonemics whose careful scrutiny might have contributed to make some of the paragraphs he devotes to the economy of phonetic systems less naively cocksure. In this respect, the argument presented in the long footnote on p. 106 is most typical: noticing that the old forms represented in Gothic by *hausei* and *hēr* have yielded in Modern English the perfect homophones *hear* and *here*, and discarding as 'ridiculous' the assumption that phonetic changes are instantaneous, he jumps to the conclusion that 'for a long or short period . . . the vowels of *hear* and *here*

were ambiguous in respect of the phonological technique.' Zipf here confuses phonetic and phonological (phonemic) changes. It is of course a well-established fact that most phonetic shifts are gradual, but it is a fact, too, which has been amply demonstrated by European phonemicists, that they often result in phonemic shifts which can be said to be instantaneous in the sense that there comes a generation of children who never learn to reproduce the subtle phonic distinction which, for their parents, was sufficient to secure phonemic autonomy. Two centuries ago, the French distinguished between the vowels of *maître* and *mettre* just as clearly as the Germans do today between those of *Kahn* and *kann*. This distinction has been gradually weakened. This reviewer, whose mother tongue is French, can always tell whether a given word has a long or a short [ɛ], though the actual phonetic distinction is a very flimsy one. His elder daughter, now 14 years old, has never learned to make the distinction, and the concepts of long [ɛ] and short [ɛ] are meaningless to her. Here is no gradualness, but a sudden shift of pattern from parent to child.

All this does not mean that the reading of large sections of Zipf's book should not be rewarding for linguists as such. The tool analogy where the author likens the dynamics of words and morphemes to that of tools on the artisan's bench opens interesting vistas. By considering at some length the problems of polysemy, Zipf suggests a healthful counterpart to the currently prevailing oppositional approach. By contrasting the needs of the speaker as such to economize and unify, and those of the hearer, which prompt the speaker to specify and diversify, he has presented the basic data which can enable today's linguists to resume historical research with a better chance to explain not only the hows, but also some of the whys of linguistic changes. When phoneticians of former periods invoked Least Effort they meant 'following the line of least resistance,' and it was an easy matter to show that fairly little could be explained thereby if by 'resistance' only inertia was meant. If, however, we include in 'resistance' the conservative pressure exerted by the necessities of communication, we shall have some chance of understanding the economy of linguistic evolution. Though the basic antinomy between conflicting needs had been emphasized before, any new presentation of it is welcome. More generally, the fact that Zipf never loses sight of the dynamics of language should recommend his writings to the far too numerous linguists of the new generation for whom static description seems to be the sole aim of their science.

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KENNETH L. PIKE. *Phonemics, A Technique for Reducing Languages to Writing*, University of Michigan Publications, Linguistics, Vol. III. Ann Arbor 1947. xvi + 254 pp. $3.00.

For the first time in the history of phonemics, we are offered a really practical handbook written for the beginners in the class-room and no longer with the more or less openly confessed aim of convincing fellow linguists of the excellence of the author's theoretical approach. Pike's *Phonemics* is thus a pioneer work which will prove a great help to all those who teach descriptive linguistics. The

author's intimate knowledge of the most varied linguistic structures has enabled him to present a very complete survey of the whole field and practical solutions for most of the problems that may arise when one endeavors to account for the phonemic pattern of a language.

The originality of Pike, as a linguist, lies in the fact that he has been reconstructing theory on the basis of an ever enlarging experience, a circumstance which undoubtedly gives to his writings a convincing ring often lacking in those of such linguists as rely too much upon their capacity to handle abstractions and not enough upon a careful observation of as many data as can be gathered.

Though phonemic theory is not absent from the book under review, it is obvious that the author consciously refrains from discussing at great length matters that are not strictly relevant to the practical problems he has to solve. Linguistics for its own sake is clearly not what Pike had in mind when writing his 'phonemics.' For him, at least in this book, phonemics is 'a technique for reducing languages to writing.' That the functional and structural approach to linguistics, of which phonemics is only a chapter, has the widest repercussions in all the domains of the science of language is a fact of which Pike is obviously aware, but which he does not choose to stress in this connection. We shall of course avoid, in the following discussion, raising any point that is not relevant to the subject as conceived and limited by the author. But consistency involves implicit adherence to some fundamental principles, and we shall not avoid referring to such as obviously underlie the author's treatment of phonemics.

One of Pike's basic assumptions is that phonemes do exist. This implies that there may exist only one phonemic pattern for a given language. If phonemic analysis, as practised by different scholars, or even by the same linguist, yields different results for the same problem, one has to assume that the existing techniques are imperfect. The conviction that our phonemic techniques are still imperfect is frequently expressed by Pike, together with the hope that a time will come when perfection will be achieved. This reviewer must confess that he is not so optimistic. If we consider, for instance, the problem of the interpretation of English [aɪ], we know pretty well all the reasons that could support its listing among the phonemes of the language, or its analysis into two successive units. As pointed out by Pike himself, here are conflicting pressures from various structural tendencies, and we do not see how we ever could avoid a conflict. On the whole, what can 'existence' mean in the case of a phoneme? We might imagine that the phoneme has a physical existence in the sense that every single actualization of it contains, among variables, the same material element. But this view is obviously rejected by Pike who, in the case of tones (66) states that '*relative pitch is linguistically significant but that absolute pitch is linguistically nonsignificant.*' Martin Joos's findings concerning the differences in characteristic frequencies with children and adults, as quoted by Pike himself, confirm the fact that even with 'segmental' phonemes, contrast and not substantial identity is relevant. Even in the speech of one and the same subject, the decisiveness of contrast can be ascertained in a case like that of Danish where, in contact with *r*, what is universally assumed to be an allophone of the ɛ phoneme is pretty

close to, if not identical with, the normal actualization [a] of the a phoneme, while, in a similar situation, a is no longer an [a] but an [ɑ].

It can be argued that phonemes do exist in the minds of speakers. It is of course bad form, and not only among convinced anti-mentalists, to mention psychological facts in connection with linguistic structure. But if there is some justification in denying scientific value to introspection, there is no reason why linguists should not try to get information through observation of the behavior of speakers. Trubetzkoy, among others, has pointed out that inferences as to the psychological existence of phonemes can be drawn from rime, alliteration, assonance, and puns. From the fact that the average Japanese speaker is apt to replace a foreign [f] by [ç] before [i] and by [h] before open vowels, it does not seem foolish to conclude that there must be some kind of mental reality for an *h* phoneme whose actualizations range from [f] through [ç] to [h]. Though many would deny it, most phonemicists are unconsciously haunted by the psychological implications of phonemic theory, and it might prove a more healthful procedure to try to tackle these problems than to stick to the current ostrich-like policy. Though we lack accurate information based upon consistent and scientific observation, it may be safe to say that the linguistic behavior of people evinces varied degrees of structural-mindedness. The diverging reactions of beginners in a phonemics class are most instructive in this respect: some take easily and even gratefully to the idea of a phonemic pattern for their native language; others, often without any previous phonetic schooling, will usually solve the conflicts between the phonetic and the phonemic approaches in favor of the former. The way children and foreigners learn a language can also be very illuminating. This reviewer once knew a Danish lady, who in the course of ten years' residence in France had acquired a good command of the language. She was gifted enough in the sense that she could give a perfect imitation of a new word, but when she used for the first time in French an international word she knew from Danish and from French writings, she would pronounce it with Danish sounds, e.g. [diafrɑɣym] for [djafragm], leaving out only the glottal catch. Yet, if corrected, she would reproduce the French pronunciation and use it from then on. In combination with the fact that the French words with which she interlarded her Danish were subjected to hardly any phonic adaptation, this would seem to indicate that she did not react to the two languages as structurally different, at least in phonetic matters, though she could at times pass for French-born.

Should we decide to base our verdicts upon the behavior of such speakers as react positively to phonemic patterning, we should probably obtain little useful information from their reactions to doubtful cases of the type that Pike calls 'suspicious sequences.' If, as is the case with Castilian [tš] for instance, the normal phonemic approach leaves no doubt as to the advisability of monophonematic interpretation, we are likely to find that the behavior of native speakers supports it categorically. When, on the other hand, standard procedures do not lead to a clear decision, we shall probably notice that the behavior of speakers is inconclusive. The reason for this is obvious: native 'feeling' and reac-

tions are determined by the same functional realities as the conclusions of the phonemicist; a life-long unconscious keeping apart of *b* from *p* so that *back* should not be confused with *pack*, *beer* with *pier*, *bun* with *pun*, etc. will leave traces in the subconscious regions of the minds of English speakers. On the other hand, the fact that *back* is distinct from *pack*, etc. is the very reason for which the phonemicist sets up the two phonemes. This, of course, affords a full justification of standard phonemic practice which confines itself to an analysis of utterances. When such an analysis does not yield conclusive data in terms of phonemes, we shall not expect to find any clue elsewhere. It probably means that the phoneme (nota bene: distinct from the phonemic) hypothesis, which appears to suit linguistic reality beautifully in most cases, does not give perfect satisfaction in a few others. If we still want to reduce all utterances to phonemes, we shall either present the various solutions which do justice to the distinctive pattern, or, in practice, let extraphonemic, mostly morphological, considerations determine our choice of one of them. The phoneme is thus nothing but a very useful working hypothesis, and our conclusion as to the nature and value of this concept cannot differ very much from the one arrived at, about 15 years ago, by W. F. Twaddell in his stimulating monograph 'On Defining the Phoneme.' We shall use the term 'phoneme' much as we do the term 'mora' merely as a tool and nothing else. We should be careful not to become the slaves of the instruments which we ourselves create.

Considering the phoneme a contrastive rather than a substantial unit has important practical consequences which Pike has not always drawn. As a matter of fact the contrastive nature of phonemic units is stressed by him only in the case of tones. It is clear that in phonemic practice reference to phonetic similarity is in most cases handier and therefore preferable to a rigidly contrastive procedure. Yet, it is only by applying the latter that it becomes possible to understand the theory of neutralization which may prove a great help in determining the most suitable writing system for a language. Pike acknowledges the existence of the problem in his section about undifferentiated phonemes where he considers the case of a language which has [i] and [ɛ] in some positions from which [ɪ] is excluded, and only [ɪ] in some others. Here of course it is difficult to decide, on the basis of phonetic resemblances, whether [ɪ] should belong with [i] rather than with [ɛ]. But when the similarity is sufficiently obvious, the author makes it the decisive factor. If we consider a hypothetical language[1] where [ɛ] and [e] are distinctive in word final position, but where in non-final only [ɛ] appears in closed and only [e] is found in open syllables, we shall have to assume the existence of two phonemes, and Pike would undoubtedly write ɛ wherever the sound is [ɛ] and *e* for all [e] sounds. Now he would probably notice that natives have great difficulty in distributing ɛ and *e* correctly, and that they will at times write *repet* for his *repɛt*, or *repetã* for his *repetã*. The conclusion should be that contrast can be more decisive than substance, particularly if [e] and [ɛ] are found to alternate in the same morphological pattern. Another case would be that of a language like Russian where [b] and [p] are distinctive everywhere except in

[1] It is a slightly simplified type of French.

syllable final position. If we now have a stem *rby-*, generally followed by some vowel, but appearing bare in a given case, we should according to Pike, write *ryba*, *ryby*, etc., but *ryp*. Now natives will probably prefer *ryb* to *ryp* because the replacement of *-b-* by *-p* being automatic, they are far less aware of the substantial shift than of the permanence of the stem *ryb-*. The practical conclusion to be drawn from what precedes is not that substance should be sacrificed in all cases wherever only one of two articulations occurs in a given situation. If we have a language[2] where [l] and [l^y] are distinct in all environments except after dorsal stops where only [l^y] exists, and if [l] and [l^y] never alternate in the same morpheme, it would be sheer perversity to write *kl* and *gl* for [kl^y] and [gl^y]. But wherever a solution can be reached which does full justice to the contrastive pattern and at the same time enables the writer to retain one and the same spelling for a given morpheme, natives are likely to take to it more readily than they would to one founded exclusively upon substantial similarity.

If, in the preceding paragraph, we have dealt at some length with the practical importance of contrast as opposed to substance it is because we feel assured that our argumentation is in general agreement with Pike's approach as illustrated for instance by his footnote 1, p. 160, where he stresses the necessity of taking native reaction into account. Though the author's treatment of phonemics is decisively colored by his confessed intention to set up a practical technique, the reader will discover that Pike's constant concern with linguistic reality in its broadest sense will go further toward elucidating phonemic problems than would the automatic application to the most varied languages of a rigid method evolved on the basis of the limited experience which is that of most of us.

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Archivum Linguisticum, A Review of Comparative Philology and General Linguistics, vol. I, fasc. 1. Glasgow (Jackson, Son & Co. Limited) 1949. 94 pp. Sh.21 per annum (two fasc.).

Though the practice of reviewing journals undoubtedly has its good points, it is not part of the editorial policy of this journal, as it is felt it involves some kind of uneconomical duplication. However, when a new linguistic journal makes its appearance, it may seem that a little more than a bare listing among publications received is not out of place.

The members of the editorial board of Archivum Linguisticum all belong to the University of Glasgow, as do most of the reviewers. But none of the main contributors to the first fascicule happens to reside on the banks of the Clyde. This is to be interpreted as a demonstration of Scottish hospitality, and not as the sign of a lack of linguistic inspiration, since the University of Glasgow is adequately represented among the authors of forthcoming papers.

Four outstanding French comparatists and philologists have sent contributions to this first issue: Emile Benveniste traces Latin ablaut perfects to two sources,

[2] Cf. the Francoprovençal dialect of Hauteville whose phonemic pattern was described by this reviewer in Revue de linguistique romane 15. 1–86; as regards the neutralization of l ~ ly, see 3–21.

(1) reduplicating perfects as in *ēmī* from **ə₁e-ə₁m-ai* which parallels *dedī* from **de-də₃-ai*, and (2) primitive perfects with full vocalic grade before *ə* on which presents have been formed by vocalic reduction and addition of *-ye-* or *-n-*, e.g. *fēcī* from **dheə₁-k-* hence *facio* from **dhə₁-k-ye-*, just as *fūgī* (**bheu-g-*) yields *fugio* (**bhu-g-ye-*). In another section of his paper, he points out that Germanic perfecto-presents denote a state of the subject, even when they may, as is the case with Goth. *aih*, for instance, be used with an object. In this, their value coincides with what can be assumed to have been that of IE perfects. J. Vendryes examines a number of words from various IE languages and points out how their having been used in the language of hunters must have influenced their semantic evolution. Such disputed etymological equations as that of Goth. *saihvan* with Lat. *sequitur*, Gr. ἕπεται are thereby shown not to be devoid of likelihood. Both German *Sinn* and Lat. *sentīre* can thus be traced back to a word meaning 'way', which affords a fine example of how abstract vocabulary can be developed or renewed. A. Ernout offers an interesting case of semantic merger of two distinct etymological groups (those of *incinō* and *incendō*) determined by partial homophony. J. Marouzeau studies the fate in Latin of the IE law regulating the place of enclitics and accessory words, and shows how a declining syntactic pattern may die hard and survive as a stylistic trick.

The distinguished Edinburgh romanist John Orr presents instances of homophonic attraction in the case of some French obscene and imprecatory words. In this somewhat marginal domain, etymological research must reckon with peculiar developments and unforeseen patterns such as the *ou-u* expressive ablaut illustrated by the alternation *bouter-buter*. This reviewer has observed, in contemporary French practice, jocular replacements of *ou* by *u*, and has heard, from a German lady, the shrewd comment that French first names that contain one *-u-* or more (e.g. *Arthur*, *Ursule*) often have something slightly comical about them.

Alan S. C. Ross, who presents J. Josephs' system of triangular tables for the determination of phonemes, seems unduly pessimistic as to the possibility of setting up the phonemic patterns of hitherto unrecorded languages and dialects.

The most interesting contribution to structural linguistics is undoubtedly that of C. E. Bazell entitled 'On the Problem of the Morpheme.' Bazell offers a very incisive and frequently convincing criticism of current views regarding the morpheme. The potential existence, for every morpheme, of a double set of alternants (expressive and semantic) is a fact which is too often overlooked by American mechanists, and which is duly stressed here, together with the feasibility of applying objective criteria to the determination of semantic variants. The statement that no variation in expression can coincide with variation in content without destroying the unity of the morpheme is obvious but welcome. As indicated by the author, there is no reason to exclude the possibility of homonymy in the case of inflexional morphemes, though identity of form within a small paradigm creates a stronger associative link than in the case of nominal or verbal homonyms. Preservative analogy (e.g. *grind-ground* supported by *wind-wound*, *bind-bound*) should not be lost sight of in morphematic analysis.

But it would lead us too far if we mentioned here all the statements with which this reviewer is tempted to agree. Other pronouncements of Bazell's such as that according to which morpheme alternants belong to *langue* (whereas allophones belong to *parole*) because 'they are recognized as members of the same morpheme by the functions which they serve,' seem far less convincing: first, it is not certain that morpheme alternants can only be identified by reference to their semantic content; second, even if this were the only possible method, the very procedure would show that the various alternants have one and the same function, so that their status is parallel to that of allophones. Bazell's paper suffers from an extreme condensation which, unfortunately, may well hamper the diffusion of the author's ideas.

Among the names of the distinguished scholars listed as contributors to forthcoming issues of *Archivum Linguisticum*, are those of J. R. Firth, Louis Hjemslev, Bruno Migliorini, and Alf Sommerfelt.

Columbia University ANDRÉ MARTINET

CALVIN KEPHART, *Sanskrit, Its Origin, Composition, and Diffusion*, Strasburg, Va., and Washington, D. C., 1949. viii + 85 pp.

This is the work of a man who describes himself as a 'lawyer, ethnographer and historian' (vi) and who states that he 'is not so optimistic as to believe that those who have spent their professional life in the field of anthropology, philology, or literature will accept all of his findings here with avidity.'

The book—an amazing and perplexing volume—is divided into two main sections, 'historical' (1–65) and 'philological' (69–86). The 'historical' section consists of the following chapters: 1. Introduction, 2. Racial Sub-divisions, 3. Nordic Homeland, 4. Nordic Nations, 5. Resettlement of the Massagetae and the Sakae, 6. Identification of the Massagetae and the Sakae, 7. Subjects of the Persians and the Grecians, 8. Scytho-Indian Empire, 9. Sanskrit Language, 10. Christianity in Central Asia, 11. Summary and Contents. The 'philological' section shows these chapter headings: 12. Linguistic Terminology, 13. Evolution of Nordic Aryan Languages, 14. Orientation of Philological Research.

The enthusiastic amateur and the interested layman are ordinarily not spurned by the specialists in most fields and, in some instances, are able to make contributions of merit in what is for them essentially a hobby. In our field of linguistics there have been some distinguished figures who were originally concerned with virtually unrelated spheres, and there have been still others who, while remaining laymen, have nevertheless participated in the development and progress of the science. Indeed, such men are not entirely absent from the contemporary scene. But in their approach they have been—although amateurs—untainted by 'amateurishness' of method. Those who are to produce fruitful results should be aware of the work of their predecessors; they should understand the status that has been attained by linguistic science; they must rigorously exclude the superstitious, the ill-founded, the biased.

Kephart's book reflects in small compass an enormous amount of reading and study and a questionable degree of digestion. Its theoretical claims are presented

with admirable assurance. There is a refreshing disregard for such pedantic devices as documentation. Footnotes, when they occur, exhibit a great predilection for the Encyclopedia Britannica, 13th edition, with an occasional nod to Herodotus. The author excuses the sparseness of footnotes in one instance (74) on the ground that 'all statements of fact or probability and inferences or deductions therefrom in this chapter are portrayed in greater detail in the writer's major research on ethnography (referred to earlier, p. vii, with charming freedom from false modesty as his *magnum opus*), soon to be published, and are well supported by citations of the findings of others.' The need for the promised volume is keenly felt, for it might provide the evidence to support the astounding information contained in the present work. We learn, for example, that 'Modern Sanskrit' was introduced into India in 58 B.C. (p. 51), that the 'basic dialect of Sanskrit was the original language of the Nordic-Aryan Tokhari before they reached Bactriana and was similar to that of the Goths' (52), that there is a Kelto-Slavic ethnic and linguistic group (84–5) to which the Poles do not belong, being classed, rather, with the 'Nordic Nations' (85), that various Hindi dialects are Iranian (84). If we need proof that there is nothing at all to the distinction between *centum* and *satem* languages, we have merely to find in the author's forthcoming book the pertinent evidence to corroborate his revelation (p. 8 of the present work) that the language of the Goths, or Getae, as subsequently modified, constituted the Sanskrit language imposed upon the Hindus of western India. And this happened not once, but twice: in 1400 B.C. invading Goths brought Vedic Sanskrit into India; in 58 B.C. the same tribes once more brought Sanskrit ('modern Sanskrit') into the same area. Kephart is impressed (6), in this connection, with the remarks of Franz Bopp 'to the effect that while reading Ulfilas' transaction (sic) of the Bible into old Gothic (but surely 'modern' compared to that which formed the basis of Sanskrit; reviewer's note), he thought that he was reading Sanskrit.'

A forthright statement is made by the author when he says (59), 'At the inception of this study it was uncertain as to where it might lead.' A sustained effort by the reviewer to find in the body of the book itself a treatment of the subject implied in the title met with scant success. An alleged discussion of the 'origin, composition, and diffusion' of the Sanskrit language manages somehow to be achieved without reference to any Sanskrit words or forms.

The terms 'race' and 'racial', whose misuse in spurious ideologies and by chauvinistic sadists has wrought unspeakable woe to great sections of humanity, play a prominent role throughout this extraordinary work, as does the related term 'blood', which, following Faust, is evidently deemed a 'ganz besonderer Saft', and which was active, in its 'Nordic' variety, in the Mogul conquest (80), for the author is convinced that a goodly portion of Nordic blood had entered the veins of those estimable conquerors. Shades of 'honorary Aryanism'! There is also implication that a certain relationship exists between language and cephalic index.

Perhaps the author's next volume will bring the promised bibliographical proof of the strange theories advanced here. If so, it will revolutionize Indic and

Iranian studies, which will take their proper places in departments of Germanics. In our present stage of ignorance we continue to believe that works in Sanskrit were written at an earlier date than that given by Kephart for the origin of that language, and even that later Indic dialects antedate the time at which Classical ('Modern') Sanskrit, according to his precise chronology, was introduced into India.

New York University ROBERT A. FOWKES

FERDINAND SOMMER, *Handbuch der lateinischen Laut- und Formenlehre, eine Einführung in das sprachwissenschaftliche Studium des Lateins.* Manulabdruck der 2. und 3. Auflage. Heidelberg 1948, Carl Winter, Universitätsverlag. xxx + 664 pp. D.M. 9.80.

A new edition of this very useful manual is being prepared by the author, but as it will take a long time before it goes to the press, it has been decided to present to the public 2000 reproductions of the latest edition, which appeared in 1913.

It is likely that many years will elapse before we get a comprehensive survey of the results achieved in the field of comparative Indo-European linguistics during the last decades. We have not yet reached a stage when people agree on many things beyond what was generally accepted before World War I. The latest edition of Meillet's *Introduction*, published in the thirties, did not present too different a picture from that which linguists were used to at the beginning of this century, at a time when the shwa was still widely supposed to have been a vowel. But it becomes increasingly difficult not to take into account the latest developments, though opinions diverge nowadays to such an extent that writing a new *Grundriss* would seem to be a pretty rash undertaking.

It is clear, however, that the new theories and hypotheses do not affect Latin historical grammar quite as much as they would, for instance, Greek. If accepted, some of Kurylowicz's views about laryngeals would entail a completely new presentation of Attic reduplication. Latin morphology, with its drastic innovations, is far less affected by revolutionary conceptions regarding the structure of the *Ursprache*. Thus, it is still possible to recommend the book under review to beginners after a serious warning against accepting every word of it as corresponding to the present state of our knowledge. Most of the chapters devoted to the internal development of Italic or Latin present a picture which is still generally accepted, in spite of recent criticisms and suggestions. In a revised edition, we should welcome a more structural treatment of many phonological items: the first step towards rhotacism should be clearly linked with the wholesale voicing of intervocalic spirants. From a didactic standpoint, it is particularly important in Latin phonology to connect with general tendencies the multitude of seemingly stray sound-shifts.

As regards pre-Italic developments, the conceptions reflected in Sommer's work strike this reviewer as definitely projecting into Common Indo-European morphological features which must have developed at some later date in the various languages. For instance, the assumption that the *Ursprache* had a fully

developed system of adjectival comparison similar to the one we find in the historical languages of the family is one which, after Benveniste's latest book, should no longer be indulged in. Suppletion, conceived as the integration into a morphological category of some old lexical terms, points to a recent development of that category. The existence of a given category in all attested languages does not give us the right to label it as Common Indo-European, unless all these languages agree in expressing it by means of the same formal elements. Few Indo-Europeanists would now accept the view that the first declension was traditionally reserved to feminines and the second to masculines and neuters. Explaining away *ā*-stem masculines and *o*-stem feminines seems to contradict the obvious trend toward specializing the first and second declensions according to gender, from a more primitive stage in which the nature of the stem had no bearing upon its gender distribution. The evolution goes from the pair *lupus* m. and f. toward *lupus* m. and *lupa* f., and not the reverse.

The preceding paragraph, which of course is not to be construed as a criticism, shows what a tremendous task it must be to revise such a work as this one, if the author wants it to keep abreast of current research. It is presented as a full justification of the decision to satisfy a demand by reproducing the old book without any change, rather than to try to patch it up by means of hasty references to the results of nearly forty years of comparative studies.

Columbia University ANDRÉ MARTINET

J. FOURQUET, *Les mutations consonantiques du germanique, Essai de position des problèmes*, Publications de la Faculté des Lettres de l'Université de Strasbourg, fasc. 111, Paris 1948. iii + 127 pp.

Fourquet's work is probably the most outstanding and far-reaching contribution to Germanic phonology published in the forties. The author, to whom we owe an excellent study of the syntax of old Germanic dialects,[1] has, for several years, focussed his attention on the problem of phonetic shifts. Being thoroughly acquainted with phonemic methods, he has become one of the small group of French scholars who have undertaken to apply them to diachronic or, as Fourquet puts it, metachronic research. He has been further served by his extensive experience as a Germanic philologist, together with an uncommon talent for balancing theoretical views with a painstaking survey of facts and a Cartesian resolution to analyze all complexes into their smallest components. This has enabled him to put the old problem of the Germanic consonant shifts in a new light which, in the opinion of this reviewer, is likely to take us much closer to a satisfactory solution of it.

One of his first tasks is to define exactly the term 'mutation' (the traditional French equivalent of German *Lautverschiebung*) as the passage from a regular system to another regular system with the same number of elements. A shift which affects only one or some of these elements is neither a mutation nor a part of one. The old neogrammarian concept of 'series' (Germ. *Reihe*) is replaced by

[1] *L'ordre des éléments de la phrase en germanique ancien*, Publications de la Faculté des Lettres de l'Université de Strasbourg, fasc. 86, Paris 1938.

that of correlation, or correlation bundle, including several series, all of which are affected by the same phonetic trend whose result is the mutation. According to the old view the first Germanic shift was a succession of particular changes affecting in turn the three (or four) initial series, with the unvoicing of IE voiced stops coming last, probably several centuries later than the change of IE surds. It had been seriously challenged by Meillet, who conceived of a delayed action of the glottis which synchronously affected both surds and voiced stops, a view strongly supported by the observation of the behavior of several modern Germanic languages. With some reservations as to articulatory details, Fourquet agrees with Meillet's conception of a parallel and concomitant shift of the two non-aspirated series, but he sees in this shift a dialectal innovation of Common IE which was shared by Pre-Germanic and Pre-Armenian dialects. Accordingly, it is no part of the Germanic mutation proper, which is a much later phenomenon of a totally different nature. Pre-Germanic dialects inherited from the period of IE unity a pattern of stops which we can illustrate by means of the labials: *ph* (Sanskrit *p*, Gr. *π*, Armenian *h*), *p* (Sanskrit *b*, Gr. *β*, Armenian *p*), *bh* (Sanskrit *bh*, Gr. *φ*, Armenian *b*). Whether we retain *bh* as such, or its deaspirated form resulting from a change common to Germanic, Celtic, Balto-Slavic, and Iranian, does not affect Fourquet's conception of the Germanic mutation. He explains the latter as the result of a general weakening of articulation. *Ph* as such is not necessarily doomed to spirantization. Wherever we do find a shift from *ph* to *f*, in Byzantine Greek, Iranian, or Italic, it is never isolated, but accompanied by a wholesale weakening of stops which tends to relax voiced lenes into spirants (*b* > *β*), and fortes into unvoiced lenes (*p* > *p̥*), though the latter feature of the mutation is not always so obvious as the former. The parallelism of the Byzantine mutation and the one Fourquet assumes for Proto-Germanic extends to consonant clusters where the positionally weak implosive consonant is relaxed, while the explosive element is not mutated: to Lat. *noctem* corresponds Goth. *naht* where the Pre-Germanic cluster *-kth-* had its implosive *-k-* weakened to [x] and its explosive *-th-* retained as a stop (with subsequent loss of aspiration in a linguistic stage where all other aspirated stops had disappeared). Compare Classical Greek *ὀκτω*, *φθίνω* with initial [pth-], and Modern Greek *ὀχτώ* [oxto], *φτίνω* [ftino]. Except as the plosive element of a cluster,[2] Pre-Germanic *ph* is relaxed to Proto-Germanic bilabial *f*, *p* is weakened to *p̥* (a voiceless lenis), and *bh* or *b* spirantized to *β*. This, the Germanic shift proper, is really, in all cases, one and the same phenomenon.

At some later period, a general strengthening sets in: *f*, which had become labiodental, cannot be affected, but *p̥*, which had remained the spirant counterpart of *t*, is ultimately shifted to a stop in Scandinavia, Holland, and Germany; *p̥* is strengthened to *p*; *β* passes to *b* except in intervocalic position where the spirant sound is widely retained as the result of another general trend. The whole evolution can be summarized as follows:

[2] In which case, according to this reviewer, differentiation of phonemes in contact may have contributed to retention of the stop.

Predialectal IE	*p*	*b*	*bh*
Dialectal IE	*ph*	*p*	*bh* or *b*
Germanic weakening	*f*	*b̥*	*b*
Later reinforcement	*f*	*p*	*b* (*-b-*)

When we pass to later mutations, namely the High German and the current Danish one, the general pattern of the language has profoundly changed, a feature which had been widely neglected before Fourquet: the Germanic mutation affects all stops irrespective of whether they appear in stressed or unstressed, initial or intervocalic position. At that time, we can still conceive of a linguistic form reminiscent of Sanskrit where all syllables have pretty much the same weight and where words are run into one another. Nothing of the sort is found in Germanic dialects and languages as we know them today or through old documents. The comparatively late shift described as Verner's Law is an indication of what the situation will be as soon as a strong initial dynamic accent develops. The High German and Danish mutations are, in their principle, not different from the Germanic-Armenian (dialectal IE) correlative shift from *p* to *ph* and *b* to *p*, but they conflict with other powerful tendencies: whereas the Proto-Germanic spirantization results from a wholesale weakening never conditioned by vocalic environment, we notice that in High German and Danish the new spirants are restricted to intervocalic and post-vocalic positions. The German and Danish mutations from a correlation of voice to one of aspiration may appear to be concomitant with spirantization, but the two phenomena correspond to totally different trends which cross each other and have to be carefully kept apart by the linguist.

Such a review as the present one can not hope to do full justice to a book packed with keen observations, suggestive comparisons, and new vistas. Before passing judgment, one must read the book carefully from beginning to end. Some views, which may be startling when first presented, will impose themselves on the reader as he proceeds. This does not mean, of course, that all is equally convincing. Fourquet's conception of IE isoglosses and his attempt at relative chronology of some dialectal IE shifts will probably meet with serious resistance on the part of many comparatists. But this is, to a certain extent, marginal to the author's purposes, and the main bulk of the work will remain as a model of phonological research and as the hitherto most lucid contribution to the solution of a problem which has attracted the attention of many of the best linguists for more than a century.

Columbia University ANDRÉ MARTINET

D. W. F. HARDIE, *A Handbook of Modern Breton (Armorican)*. University of Wales Press, Cardiff 1948. Pp. 241.

As pointed out in the preface (viii), this is the second grammar of modern Breton to be published in a language other than French. The other such work, *An Introduction to Breton Grammar* by P. Treasure, was, like the present volume,

published in Wales (Carmarthen 1903). It was, however, a sketchy treatment of no scientific pretensions.

Hardie's book, altho largely descriptive, is not only of value to those wishing to acquire a knowledge of the modern literary language (for which purpose the book appears eminently adequate) but also is of considerable interest to linguists in the field of comparative Celtic grammar. As is to be expected, constant parallels are drawn between Breton and Welsh, a feature missing in French works on the language.

Before discussing the modern language (by which Hardie means the northern dialects and the literary tongue based on them; the dialect of Vannes is omitted because of its allegedly great variance from the others), the author gives a brief history of the language and its literature (3–23) and also mentions earlier grammatical treatises. The mutations are presented both descriptively and historically (with linguistic reconstructions, 28–40), and mention is made of the striking similarity of certain features of Celtic mutation and the second Germanic sound-shift. (Why the first shift is not mentioned is puzzling to the reviewer.)

Chapters eleven to thirteen, altho traditional in method and terminology, present clearly and carefully a full discussion of Breton grammar. The examples used as illustrative material are all taken from modern Breton texts (nineteenth and twentieth centuries), hence the spurious idioms often found in Celtic grammars do not occur in this book.

Chapter fourteen consists of a Breton folk-tale, Benediste, with ample notes to aid even a beginner to read the selection.

A short but valuable bibliography of works on the Breton language, largely grammatical, and partially linguistic, is included (200–203). A very useful, compact Breton-English vocabulary (205–234) and a note on Breton prosody (235–6), followed by a general index, complete the volume.

Perhaps the least successful section of the book is that treating pronunciation (25–8). This is decidedly too brief and neglects all phonetic (not to mention phonemic) presentation. The familiar and useless method of equating sounds with alleged equivalents in other languages (the inevitable Scotch *loch* occurs) is employed here.

A few qualitative judgments patently undemonstrable by objective scientific criteria occasionally mar the generally high quality of the work. One such statement follows, 'To demonstrate, however, the inadequacy of a Celtic language to express the concepts of some forms of modern thought, let the Gael render in his language the Three Laws of Motion, for which Newton found scholar's Latin sufficient.' Speaking, not for the Gael, but for the Cymro, with, however, the same implications, one need only refer to recent Welsh works on astronomy, chemistry, the atomic theory, and the like to refute this supposed impotence of the Celtic tongues.

All in all, this seems to be the most satisfactory grammar of modern literary Breton yet published, but for certain linguistic aspects of the subject one still looks forward to the historical and comparative grammar of Breton being prepared by W. S. B. Smith (cf. L. H. Gray, *Foundations of Language* 344). The

reviewer is quite certain that Smith's discussion of dialect relationships, for example, will vary considerably from that in Hardie's work and will pay more attention to modern techniques of linguistic geography. In fairness to Hardie, one must add that his work is based exclusively on the written language, the legitimacy of which procedure he attests by the following quotation from Meillet: 'Il (the written language) a le mérite de mettre en évidence dès l'abord, non des accidents individuels et momentanés, mais une norme, car la langue écrite est fixée et reproduit en général le type idéal auquel tous les sujets parlants visent à se conformer.' But the norm and the conformity referred to by Meillet are not always the most fruitful fields for linguistic exploration.

New York University ROBERT A. FOWKES

CORRECTION

In our article, Notes on the French Phonemic Pattern, 5.158, for the phoneme *m* (occurring five times) in the sample transcribed analytically, the fourth feature, Gravity versus Acuteness, should be corrected to read + instead of —, as is clear from the article and diagrams.

Roman Jacobson and *John Lotz*

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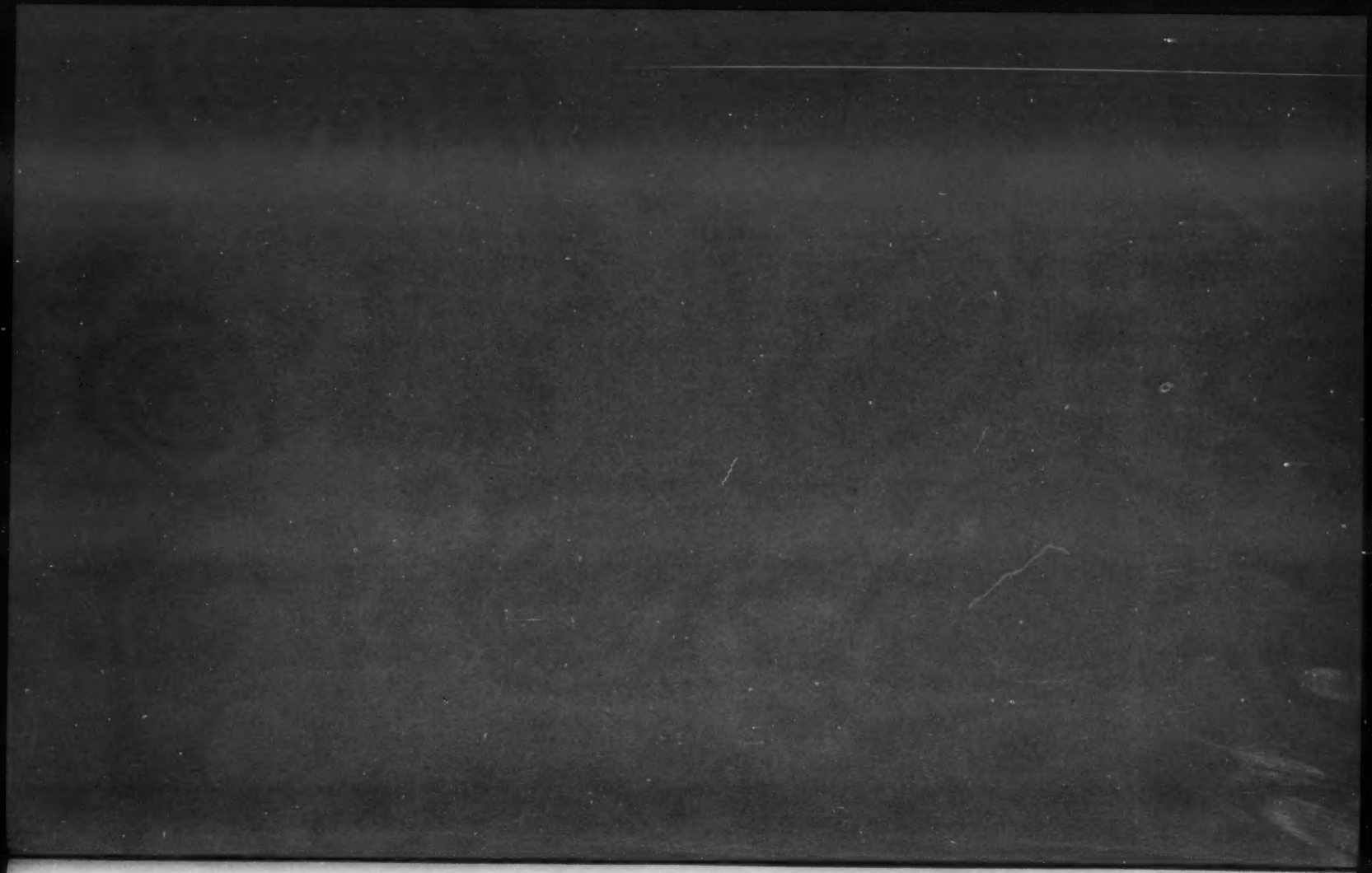

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